Thos N. Jenkins

FATIGUE AND ITS EFFECTS UPON CONTROL

BY
ISAAC EMERY ASH, PH.D.

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PREFACE

THE work here reported has been done under the personal direction of Professors V. A. C. Henmon and Daniel Starch, and to these men the writer wishes to acknowledge his obligations for numberless suggestions and criticisms without which it would have been difficult, if not impossible, to accomplish the results here presented. Grateful acknowledgements are also due those who have given time in making the experiments.

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FATIGUE AND ITS EFFECT UPON CONTROL

PART I. PHYSICAL FATIGUE

I. The Problem of Fatigue

Fatigue is a comprehensive term which in its widest application embraces all those immediate and temporary changes, whether of a functional or organic character, which take place within an organism or any of its constituent parts as a direct result of its own exertions, and which tend to interfere with or inhibit the organism's further activities. Its principal effect is loss of efficiency, a lessening of the capacity to do work or to sustain activity; its most obvious sign is depression,—a lowering of sensitivity so that a given stimulus calls forth a response of less magnitude and intensity after exertion than before.

If, however, efficiency and sensitivity to stimuli were always inversely proportional to the amount of work done, *i. e.*, if particular units of activity always represented a definite loss of efficiency, the problem of measuring efficiency, or its opposite, fatigue, would be a relatively simple one. The amount and quality of work which an individual can accomplish at any time is determined by a large number of factors. Temperature, light, atmospheric conditions, time of day, distractions, mental attitude, practise and familiarity, health and nutrition all exercise a very decided influence upon the state of one's energies and his ability to direct them to the accomplishment of specific tasks. Therefore, since fatigue is dependent, not only on amount of work actually accomplished, but also on the conditions under which it is done and the physical and mental state of the worker, the fatigue state will have to be estimated on the basis of changes within the organism rather than on the basis of what it has actually accomplished.

In the following pages I shall attempt to show that one of the chief symptoms or characteristics of natural fatigue, and one which represents most uniformly the relative effects of varying amounts of exertion, and hence furnishes the most reliable basis for estimating fatigue, is a loss of control in the execution of specific movements or in the performance of particular and highly specialized mental functions. In doing this I shall first state briefly and as concisely as possible the main principles upon which this conclusion is based and then review certain experiments made to demonstrate these principles and verify the conclusion.

The historical sections of this study will be both brief and fragmentary. They are included not for the purpose of giving a systematic review of the work which has been done in the general field of fatigue, but only as they have a definite bearing on the specific topic under consideration. In view of the fact that "there remaineth yet very much land to be possessed," it would seem that the first and most pressing need is the exploration of new territory, surveying and mapping out that which has been acquired only to the extent that it will aid in determining the most advanced position from which we may safely direct further operations. Hence, if this study should seem to omit some of the most important facts and principles that have been developed in connection with the general subject of fatigue, or to pass over works of particular significance for an exhaustive presentation of the entire problem, it will be because their application lies outside the specific field of the present investigation.[1]

Although this study is intended to deal primarily with the question of mental fatigue, the first inquiries will be directed toward finding out some of the fundamental principles that underlie the phenomenon of physical or muscular fatigue in the hope that these principles, though they may not apply entirely to the former problem, may perhaps at least suggest a clue by which it may be successfully attacked. Owing to the fact that muscular activity is susceptible of more exact and direct methods of experimentation than mental activity the phenomena of muscular fatigue are more exactly known than those of mental fatigue.

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[1] There have been published within recent years a number of works giving concise and withal comparatively comprehensive reviews of the work that has been done in the general field of fatigue, at least in so far as that field has been experimentally covered. Among the more representative of these may be suggested the following: Burnham: "The Problem of Fatigue," *Am. Jour. of Psych.*, Vol. XIX.; Yoakum: "An Experimental Study of Fatigue," *Psych. Rev. Mon. Sup.*, Vol. XI.; Lee: "Fatigue," Harvey Lectures, Vol. I.; Offner and Whipple: "Mental Fatigue"; Claparède: "Experimental Pedagogy," Chap. 5; Burgerstein und Netolitsky: "Handbuch der Schulhygiene." The last three deal primarily with mental fatigue. While none of them touch all phases of the subject, yet, taken together, since they approach the problem from various viewpoints, they will suffice to give the general facts so far as they have been worked out experimentally.

II. The Locus of Muscular Fatigue

As the term ''muscular fatigue'' would indicate, fatigue which results from physical exertions was formerly thought to be a condition of the muscles only. It has been repeatedly shown that if an extirpated muscle be electrically stimulated it will contract in the same manner as an intact muscle contracts from voluntary stimulations, *i. e.,* from effort, and that if these contractions be repeated for a time in sufficiently rapid succession there will take place within the muscle tissues certain chemical changes. The carbohydrates contained within the tissues will be decomposed and there will be formed certain toxic substances, particularly lactic acid, creatin, and carbon dioxide. That these latter substances tend to inhibit further contraction is demonstrated by the fact that when they are removed by flushing the muscle with defibrinated blood, a weak salt solution or any other cleansing substance which of itself is not injurious, further contractions of the muscles can be secured.

Weichardt (34) claims to have isolated from the muscle tissues of living animals, fatigued by their own exertions, these fatigue toxins and also to have shown that if they be injected into the body of a rested animal they will produce there all the symptoms of natural fatigue. Mosso (22) transfused the blood of a dog exhausted by continuous running into the vessels of a rested dog from which an equal amount of blood had been withdrawn. This latter dog exhibited all the usual phenomena of natural fatigue.

The experiments of Lee and Harold (15) (16) have been the most conclusive in establishing the value of carbohydrates in sustaining muscular activity, by showing the effects of the loss of these to the muscle tissues through causes other than physical exertion. They allowed cats to fast for several days and during the latter portion of this fasting period, administered hypodermically considerable doses of phlorizin, which decomposes carbohydrates and removes them from the body without injuring the muscle fibers. At the end of a period which other experiments had shown to be adequate for the removal of this latter substance from the tissues of the body, the animals showed extreme muscular weakness. They were then killed and the contractions of certain muscles electrically stimulated were recorded. It was found that because of this artificial destruction of the carbohydrates, the muscles were capable of making only one fifth to one third the number of contractions of which a normal muscle is capable, and also that the contractions actually made were weaker and of less magnitude than those of the normal muscle.

The effects of muscular exertion upon the nervous system are less susceptible to direct methods of study than those upon the muscles themselves. While it has been apparently demonstrated that nerve fiber performs its functions entirely without fatigue, there are many who claim that there are other parts of the nervous system which yield to the effects of fatigue while the muscles are still capable of continuing their work. The investigations of Abelous (1), Santesson (23), Joteyko (13), Waller (31) and a number of others have shown that when a muscle in fatigue ceases to respond to stimuli sent to it through its nerve it will still respond quite vigorously if the stimuli be applied to it directly. Their inference is that the motor nerve end-plates within the muscles are the first parts of the organism to yield to the effects of fatigue. Their theory is that fatigue of the motor end-plates serves as a protection to the muscles against the complete impoverishment of their energizing substances and the extreme toxic poisoning that would result if each motor impulse were allowed to reach the muscle with no diminution of intensity.

Mosso (21) contended that natural fatigue, the result of voluntary exertion instead of artificial stimulation, is not a peripheral but a distinctly central phenomenon. He with Lombard (17) and Waller (31) claimed to have found that when a set of muscles, for example the flexors of the fingers, is fatigued by voluntary contractions, while lifting a given weight, to such a degree that they cease entirely to respond to the most intense efforts to contract them, they will respond very readily to electrical stimuli if applied directly to the muscles themselves. While such fatigue might be assigned to the motor nerve end-plates, Mosso held it to be primarily a condition of the central nervous system; and that its primary cause is a depletion of the vital or nervous energy of the entire organism. Kraepelin (14), Hough (10), Woodworth (35), Storey (28) and Joteyko (13), claim, however, that in these experiments the muscles that were stimulated by volition and those that were stimulated electrically were not the same muscles, and that such experiments actually show nothing whatever as to the locus of fatigue. Whatever the facts of these experiments may be, Mosso (21) and a number of other investigators following him, claim to have proved that the muscles of the finger in lifting a given weight are fatigued much more readily if the subject has just previously been engaged in any kind of vigorous exercise whether physical or mental.

Other adherents of the theory of central fatigue or fatigue of the central nervous system base their conclusions on certain histological changes which occur within the bodies of the nerve cells during activity. According to Hodge (9), Vas (29), Mann (18), Eve (7) and others who have studied these changes, moderate activity is accompanied by an increase in the bulk of both cytoplasm and the nucleus of the cell body; while vigorous or prolonged activity causes a decrease in the bulk, and the appearance of vacuoles in both, and also a loss in the substance of the Nissl bodies. There are those who, though not denying that these changes in the bodies of the nerve cells do accompany physical exertion, deny that nerve centers are involved in any functional change which occurs within the organism as a result of its own activities or exertions. Woodworth (35) found no difference in the rate of fatigue of the two gastrocnemius muscles of a frog when one of them was stimulated directly and the other indirectly, either through the medulla or through a sensory nerve and the nerve center which served as a union between it and the sciatic motor nerve. Mlle. Joteyko (13), experimenting also on the muscles of the frog, arrived at the same conclusions as Woodworth, but by a somewhat different method. She destroyed the brain of the animal, thus making of it a purely reflex machine, and then exposed both sciatic nerves. By stimulating one of these nerves she elicited direct contractions of its gastrocnemius muscle and reflex contractions of the corresponding muscle on the opposite side. She then blocked the passage of the impulses in the second nerve just before they reach the muscle, and continued to stimulate the first nerve until its muscle was exhausted. She then removed the block from the second nerve and permitted the impulses to pass into its gastrocnemius muscle, which then responded almost as rapidly and as strongly as at first; and the contractions continued quite as long as if the stimuli had been applied directly to the motor nerve on that side instead of indirectly through the various sensory nerves and reflex centers which lie between the first and second motor nerves. She concluded with Woodworth that if the reflex centers are subject to the influences of fatigue they would have shown it by an increased resistance or a loss of functional power caused by the transmission of the impulses through them while the first muscle was being exhausted. Instead of this, however, they gave no evidence of having lost any of their functional capacity, whatever.

Professor Sherrington (24) (25), of the University of Liverpool whose researches into the action of the nervous system have extended the knowledge of that branch of physiology farther perhaps than those of any other investigator, does not believe that central fatigue is due to conditions or changes within the bodies of the neurons. His theory is that the connection between the different neurons is not one of continuity but of contact. This point of contact he terms the synapse, and ascribes to it the first incidence of fatigue. One of his experiments by which he demonstrates this is as follows: A certain nerve center within the spinal cord is selected which forms a union between a number of afferent nerve tracts and a single efferent tract to a given muscle. One of these afferent tracts is stimulated and the stimulus passes through the common nerve center and over the common efferent tract and is thus neutralized in the contraction of the muscle. After a short time this muscular contraction weakens and finally ceases altogether. The stimulus is then applied to another of the afferent tracts and the motor response of the muscle is as strong as at first. Now since nerve fiber is known to be practically unfatigueable, and since the same nerve center, motor end-plate and muscle are involved in both series of stimulations, it is evident that the fatigue resulting from stimulating the first afferent nerve can not be ascribed to any of these; hence Sherrington concludes that it must be ascribed to the synapse where the afferent nerve tract comes into contact with the nerve center. Professor Sherrington has also shown that if a particular spot on the skin of a "spinal animal" be stimulated either mechanically (*i. e.*, by pricking or scratching), or electrically, it will elicit certain reflex movements, notably the "scratch reflex"; but just as in the former experiment, in which the afferent nerve tract was stimulated directly, this reflex movement will presently cease. But if a second point of stimulation be selected a short distance removed from the first, the movement will be immediately resumed. The fatigue from the first stimulations might in this case be ascribed to the sensory nerve endings within the skin. Sherrington, however, does not accept this view, but, on the basis of his results in the previously cited experiment and of others which need not be detailed here, he concludes that it is to the synapse, where the sensory neuron comes into functional contact with the motor neuron, that this initial fatigue must be ascribed.

If, in the last experiment, when the reflex movement from the first stimulation ceases, instead of shifting the place of stimulation to another spot on the skin he increases the intensity of the stimulus, the movement will be resumed, but it will be slower and feebler than at first, and there will also be contractions of other adjacent muscles. If the stimulations be continued without interruption and their intensity gradually increased, the first reflex can be maintained for a considerable time, but it will grow gradually feebler and slower, and other muscles more and more remote will be brought into action. In this case the impulses resulting from the increased stimulus are "blocked" by the resistance which fatigue has raised at the first synapse, until, by the summation of successive impulses, sufficient "force" or "energy" is developed to overcome this resistance. But in doing this these "summed" impulses accumulate sufficient force to overcome other resistances greater than would be encountered in their first pathway were fatigue not present, and are partly shunted off into other pathways and neutralized in the contraction of other muscles.

McDougall (19) calls attention to the same phenomenon in connection with experiments in voluntary muscular contraction, particularly experiments with the ergograph. He says: "The subject of the experiment has to lift a heavy weight repeatedly at brief intervals, by bending as far as he can his middle finger. Fatigue very soon manifests itself in the form of a diminished extent of the bendings of the finger. Now if you carefully observe the subject as he continues to repeat his efforts, you will usually see that, while at first he contracts only the muscles immediately concerned in the flexion of the finger, other muscles come into play as the flexions become diminished in extent; first the muscles of the upper arm, then those of the shoulder, then those of the trunk, still later the muscles of the lower limbs and of the arm and shoulder of the opposite side, and even those of the face, jaw, and neck on both sides. There takes place, in short, a spread or march of excitation from the motor tract directly concerned in producing the flexions of the finger to adjacent motor tracts, then to those successively more distant. The impulses overflow, first into the most nearly connected tract; later into those successively more remote, as the resistances of the more nearly connected tracts become in turn increased."

Professor Sherrington likens the synapse in its function to the motor nerve end-plates,—the former constituting a kind of safety fuse within the central nervous mechanism; the latter playing a similar rôle within the muscles. Of the two he believes the synapse to be the more susceptible to fatigue.

We shall now leave this particular phase of the subject and pass to a consideration of the more general conditions of fatigue as it is developed within an organism all of whose members are intact and whose activities are the results of its own volitions. If our inquiry has thus far established anything conclusively it is that fatigue is not a single entity but a complex of a number of different factors, each one operating in its own peculiar way and producing its own results, and that it is a fusion of all these results which we term organic fatigue. We have seen that the amount of work which a set of muscles, contracting under the influence of artificial stimuli, will do is determined by at least three factors,—the tone or condition of the muscle itself, the intensity of the stimulus and the resistance which the stimulus must overcome before it reaches the muscles. Fatigue was represented by the difference between intensity of stimulus and the strength and magnitude of muscular response.

The essential difference between the problem of determining fatigue as it is developed under such restricted and controlled conditions and that of measuring fatigue as it develops under normal conditions in an organism working as a free agent, whose stimuli are its own volitions and whose impulses to action are supplied by its own free energies, is that in the former case we have simply to calculate the ratio between two objectively measurable magnitudes, while in the latter we have to deal with a number of indeterminate and yet obviously variable factors. We can not measure effort or volition; nor can we gauge organic energies as we can the strength of an electric current. In fact, the only thing whose measurement has been attempted in connection with the activities of a free acting organism has been the amount of work accomplished and that only under restricted conditions.

When we attempt to apply the principles established by a study of the activities of single isolated muscles in interpreting the behavior of an entire organism we encounter conditions which make our deductions only postulates. We know from experimentation that certain toxic poisons form within the muscle tissues during activity. We also know that our own muscles become sore and sluggish during vigorous or long-continued exertion. We infer that this soreness and sluggishness are caused by these toxins in the muscles. We know from experiments that the repeated transmission of motor impulses from a nerve center over a motor nerve tract to a muscle develops a resistance at certain points within the motor tract. We likewise know that a tired muscle responds less readily to our efforts to contract it than does a rested one and we infer that the motor impulses are in a measure neutralized by resistances encountered at certain points. It has been determined experimentally that the nerve centers undergo certain organic changes during activity. We know from experience that exertion of one part of our organism lessens in a certain measure the efficiency of other parts. We conclude that the central nervous system is the seat or storage battery for organic energies and that our activities have depleted those energies in proportion to the extent of the activities.

We may accept all these hypotheses at their full face value (and there seems to be no sufficient reason why we should not), and yet be far from a solution of the specific problem of measuring fatigue. We have so far only found the terms, or a part of them at least in which the unknown quantities of the problem are to be expressed. Their value is undetermined.

It may be suggested that since fatigue represents a complex of causes all operating in such a way as to bring about the same result it is not necessary to evaluate all the factors separately but simply to take the results of all *en bloc.* This would be entirely plausible if a living organism were a standardized machine whose potentiality or efficiency is always inversely proportional to the work it has accomplished within a specific period. But as we have seen there are a large number of influences which may affect the organism and determine its efficiency. It is a fact of common experience that one engaged in almost any kind of work may struggle along for a time accomplishing very little and that at the expense of a great deal of effort, and then all at once seem to 'strike his stride' and proceed with his work very efficiently with very little apparent fatigue. It is in fact just this sort of condition which renders the problem of fatigue so complex. How are we to account for this increased efficiency in the midst of activity. Do the working organs suddenly acquire tone and immunity to the toxins of fatigue, or are the resistances to motor impulses suddenly removed or are fresh stores of organic energy made available. To seek an answer to some of these questions is the purpose of the next section of this study.

III. CONDITIONS OF FATIGUE

Without going in detail into the various theories of the manner in which such things as interest, practise, the presence or absence of distractions, familiarity, and the host of other conditions may influence one's working efficiency or affect the ratio between active energy and responsiveness of working organs, we shall touch upon a few of the explanations of the processes by which the potential energies of a living organism are liberated and made to function in activity, and suggest some places where these processes are thought to occur. It will be recalled that the earliest explanations of physical fatigue treated it as a phenomenon affecting only the muscular system. Muscular activity caused the destruction or decomposition of certain substances of value to the organism and the formation of waste matter which inhibited further activity. The process was somewhat analogous to that which takes place within a furnace. The combustion of fuel releases energy and also causes an accumulation of waste or refuse in the form of ashes and soot. The capacity of the furnace will depend upon the relative quantities of fuel and refuse and the condition of the drafts and other factors which influence combustion. So muscular energy will depend upon the relative proportions of energizing substances and waste matter which the muscles contain. Practise and familiarity with the work enable one to conserve and properly direct his energies. Interest will act somewhat as the drafts of a furnace, while health, nutrition, etc., will determine the supply of energizing matter, and the ready removal of waste products.

There are others who hold that muscular fatigue does not represent so much an actual loss of muscular efficiency or muscular power as it does a loss of control of will power over muscular action. It is claimed by Wundt (37), James (11) (12), Baldwin (2) and others that all feelings of the amount of effort required to make muscles contract is dependent on impulses reaching the psychic centers from the muscles, tendons and joints. Muscular contraction becomes sluggish and painful, and the curve of effort rises as the curve of muscular performance falls. Fatigue is represented, figuratively, by the distance between corresponding points on this curve. Interest, excitement or pleasurable anticipations may supplement will power and thus enable one to continue his activities longer than under humdrum conditions. Practise makes work easier because in a sense it makes it habitual. Each successive effort becomes easier just as the resistance to temptation to act in any particular manner becomes easier the more often it has been resisted. But in the last analysis fatigue is represented by the degree of control which will power, supplemented by these other factors, can exert over the action of motor organs.

It will be seen that these explanations of the conditions of fatigue do not regard the nervous system as in any way involved in its development. The nervous system is neither the storehouse of organic energy nor does it exercise any control over the disposition of the energies supplied by other organs of the body. It merely conveys impulses, or as Deschamps (6) expresses it, "the nervous system merely distributes the active energies supplied to it by *stimuli* of all kinds and by the various organs of the body."

By most recent writers on the subject, these explanations of the conditions of fatigue are not accepted. Normal fatigue is due primarily to processes going on within the nervous system. It is within the bodies of the nerve cells that potential energies are changed to the active state and that stores of potential energy are consumed. It is here also that the waste products of activity are formed and exercise their inhibiting influence. The essential difference between this view of the conditions of fatigue and that which ascribes it to processes taking place within the muscle tissues is not one respecting the character of the phenomenon but as to the place at which it occurs. In both cases fatigue is held to be the result of the decomposition of substances of value to the organism and the formation of waste products. The former is a strictly local process affecting only the muscles involved in any activity; the second is both local and general, because the cell body may be poisoned not only by the waste products of its own activity, but also by those formed in other nerve cells and of muscle tissues also; since these waste products wherever formed may pass into the blood and lymph and thus be carried to all parts of the body. It is for this reason, according to Verworn (30), who may be styled the chief protagonist of this theory, that fatigue induced by the activity of a single organ may become general for the entire organism. Interest and anticipation will supplement effort by making available ''deeper and greater'' stores of potential energy; while practise and familiarity with one's work enable one to conserve his energies by eliminating all superfluous and random movements. Practise also renders the organism more immune to the toxic products of activity.

McDougall (19), who accepts Verworn's theory that the nerve cells are the seats of the liberation of organic energy, and that available energy is represented by the ratio which obtains between the waste products of metabolism and available energizing substances within the bodies of the nerve cells, contends that this represents but one side of the problem. Basing his theory largely on the results of Sherrington's investigations, he places great emphasis upon the resistances which the repeated discharge of neural energy over the nerve tracts raises at certain points in the nerve pathways. He defines fatigue as ''a state in which the resistances to the active energies is raised above its normal value, either through increase of the magnitude of the resistances or through diminution of the quantity of disposable energy.'' With Sherrington he ascribes to these resistances a protective function. He says: ''They prevent the organism exhausting its stores of potential energy through unduly prolonged, intense, or widespread activity.''

According to this scheme the magnitude of resistances varies directly with the amount of work accomplished, or rather with the number and strength of the motor discharges across the points at which they are developed, *i. e.,* the synapses. The quantity of disposable energy may increase, however, as work progresses, since a developing interest in, and a pleasurable anticipation of the outcome of the work will serve to bring into play greater stores of potential energy. Hence, though resistances do increase regularly as work progresses, yet, since available energy may increase more rapidly, the ratio between the two will decrease, and the organism's objective efficiency be greater after a period of work than before.

All these theories of the "conditions of fatigue" place great emphasis on the influence which interest, anticipation, suggestion, and all the other factors which go to make up the mental attitude of the worker, may exert on the relative fatigue effects of different kinds of work, or the same work under different conditions. Interesting work is easier because interest makes available greater stores of energy, or because it supplements effort or because it aids those chemical or physico-chemical processes by which potential energies are changed to the free active state. Without questioning any of these theories or denying the explanations which they offer, the writer believes there is another sense in which interest and allied factors may influence the ratio of available energy to the task at hand, and especially the relative fatigue effects of drudgery and of fascinating employment.

If one is engaged in a bit of intensely interesting work, all his sensibilities are absorbed; irrelevant suggestions and the play of external stimuli fail to make any impressions or arouse any impulses. It is as though all his sense organs or the perceptive faculties were insulated against extraneous influences; or as though they were detached from the functioning organs. On the other hand, if work be uninteresting, every casual stimulus makes an impression and elicits some kind of conscious response. Trains of irrelevant thought are constantly being set a-going and require suppression, while extraneous suggestions keep clamoring for the center of attention.

Now it is one of the corollaries of Newton's second law of motion that as great a force is required to stop a body moving freely at any given velocity as would be required to give it that velocity from a state of rest. May not the same thing be true with respect to the impulses which outside stimuli and irrelevant suggestions arouse in one who is unable to keep himself sufficiently absorbed in his work to exclude such influences? Each one of these things succeeds in making an impression. Every thought, suggestion or desire arouses an impulse to some action or mental attitude which is equivalent to a discharge of energy or the releasing of a force which requires another equal force or a like amount of energy to counteract.

The condition is somewht analogous to that which obtains when one attempts to balance himself in an unstable position. Such a balance is not maintained by relaxing the muscles, but by keeping the tension of those which support from one side equal to the tention of those which support from the opposite direction. And if one thus situated feels himself suddenly losing his poise he does not attempt to regain it by relaxing the muscles on the side on which he feels there is the greater tension or toward which he seems to be falling but by increasing it on the side on which it is least. So it is with one working in the midst of distractions. He cannot prevent these distractions from arousing impulses, nor can he permit these impulses to take their normal course. Each impression gives rise to an impulse which requires another of equal strength to counteract it. For each impulse thus aroused and action inhibited there is required the expenditure of twice the amount of energy that would be required if no attempt were made to inhibit the act or the mental attitude toward which the impulse is impelling. Under such conditions the organic energies are being drawn upon through three distinct channels: First, there is the demand for the energies necessary for the prosecution of the work in which the worker is voluntarily engaged; second, the play of all these casual and irrelevant stimuli which succeed in making an impression, by the very fact that they do make an impression, thus cause certain reflex impulses whose arousal means the liberation or expenditure of certain quantities of potential energies; lastly, the voluntary arousal of inhibitory impulses necessary to counteract these reflexive impulses causes an expenditure of energy equal at least to that by which these latter are generated. If the work at hand is of a sufficiently absorbing character both of these latter demands upon the potential energies of the organism will be avoided.

A very simple experiment, by which the fatigue resulting from such repression and the inhibition of activity may be very effectively demonstrated, may be made as follows: Allow the observer to take the most comfortable position he can assume, whether sitting or reclining and then with his attention constantly fixed on his position and his purpose not to move a single muscle see just how long he will be able to maintain that position. The writer has found himself and has seen others as completely exhausted after twelve or fifteen minutes of such effort as if the time had been spent in the most vigorous activity.

If we accept the principle that physical activity results from the liberation of potential energy by the combustion or decomposition of certain energizing substances, whether within the bodies of the nerve cells or within the muscle tissue and if we regard this latter process as being caused by the action of various stimuli or impulses received and transmitted by the nervous system to the various seats from which free energy is liberated, it is quite evident that there must be points on the nervous system at which by some means the organism may direct the course of these impulses and determine where and how they shall function. Otherwise, instead of the orderly sequence of nervous excitation which normally follows any stimulation, the excitation would spread indefinitely throughout the entire nervous system and produce an incoordinate general activity. Moreover, if every stimulus, whether of volition or the result of some external force playing upon the sense organ, were transmitted in its full intensity to the points from which active energy is liberated, there would be no means of protecting these sources of energy from a condition, if not of absolute exhaustion, at least closely approaching it.

In all the explanations of the condition of fatigue which take into consideration the effects upon the nervous system as an organ for the conduction of impressions and impulses, emphasis has been laid entirely upon the resistance which fatigue raises to their free transmission. Nothing has been said of its effect upon the ability to inhibit undesired actions. While fatigue raises the resistance to the free transmission of impulses along certain desired pathways, it also makes it necessary for the organism to raise the resistances in other pathways to compensate the former and thus prevent random and incoherent movements.

Sherrington's experiments, it will be recalled, demonstrated that the repeated passage of a nervous impulse from one neuron or nerve unit to another, developed at their point of contact a resistance which either inhibited the transmission of other impulses over the same pathway or materially reduced their intensity. In a normal animal, however, we do not meet with such a gradual and regular spread of excitation or shunting of impulses as is found in the spinal animal. When the nervous system is intact there is present the ability to raise the resistances voluntarily and compel the impulses to travel over their initial pathways or any others that the organism may desire. It is this factor of control over the direction of motor impulses which a normal organism is able to exercise which makes the problem of determining the degree of fatigue so much more complex than in one in which the centers of control have been severed from the motor organs. In natural fatigue, we should have present two conflicting tendencies: First, there would be the tendency for the impulses to be shunted off by the resistances raised by fatigue in nervous tracts whether at the synapses or in the motor nerve end-plates; and second, the tendency of the organism to try to control the direction of impulses by voluntary inhibitions in adjacent nerve tracts.

If then, fatigue represents a conflict between two opposing tendencies in the organism respecting the course which motor impulses shall take, we should expect the first symptoms of fatigue to be a loss of control in directing activities and in executing movements so as to make them coordinate with volitions. The effects of fatigue, according to this conception of it, are similar to those of a number of other disorganizing agencies and influences which affect an individual's efficiency. They simply set one back, so to speak, in his process of development, and in the acquisition of efficiency for particular kinds of accomplishments. And since control in coordinating activities and directing movements is acquired last in the process of developing efficiency it will be the first to fall away under the influence of those disorganizing factors which interfere with efficiency. In other words, the ability to execute movements will remain after one has lost the power to control and coordinate them.

The first stages of alcoholic intoxication are marked not so much by an inability to execute movements nor by a decrease in the force of such movements, as by an inability to direct one's energies to the execution of specific kinds of controlled movements. The athlete who has become greatly excited, or "rattled" as it is termed in sporting parlance, has not necessarily lost any of his gross physical efficiency if measured by the force which he can "put behind his movements." What he has lost is the ability to coordinate these movements in such a manner as to accomplish his purposes. The smith or carpenter does not deal his blows with less force at the close of his day's work than at its beginning, but far more of them are misplaced. The shambling unsteady gait of the traveller at the close of a long day's journey, in contrast to the firm and steady step at its beginning is the result of a loss of motor control rather than a depletion of energy.

The primary function of practise and training is not to develop organic energy. That is dependent rather upon the conditions of health, nutrition, etc. The two principal results to be accomplished by training are a greater immunity of the organism to the toxic products of fatigue and an acquisition of the ability to conserve one's energies and direct his impulses to the execution of only such movements as will be of assistance in accomplishing the specific ends. It is quite natural that these two processes should go hand in hand since the primary function of the resistance to nervous impulses is the protection to the muscle tissues from excessive toxic poisoning. Hence, as their immunity increases, their need for such protection will decrease.

If we observe an athlete through a course of training for some specialized kind of activity, such, for example, as would be required of an oarsman or a canoeist, we may see the conflict between the subject's attempts to control the direction of motor impulses and the tendency of these impulses to shift their course carried out to a final issue. After the first practise period, if it has continued for a considerable time, the subject will experience fatigue and soreness in nearly all his muscles, since, owing to his inability to control the direction of his motor impulses, these impulses have spread to all parts of the body and caused the exertion of the entire muscular system. But as training progresses his control will increase until finally only those muscles will be brought into action whose contractions aid him directly in accomplishing his desired ends.

Let us suppose that the practise periods have been one hour daily for several days. At first, as we have said, his whole muscular system will be fatigued, but later only those muscles will be fatigued which are directly involved in making his desired movements. If now, however, the length of the practise period for any single day be doubled or if it be gone through with under conditions of intense excitement or anxiety, the same effects will be experiencd as at the beginning of training. A member of an athletic team or a boat crew feels much more fatigued after a real contest, especially if it has been close and exciting, though of much shorter duration and objectively less strenuous, than after his practises, since conditions in a real contest are less favorable for exercising the degree of control necessary to conserve his energies.

IV. Experiments in Muscular Fatigue

Since practise and fatigue represent diametrically opposite results, it is evident that the disorganizing effects of the latter as expressed in the loss of control in coordinating movements will first be expressed in connection with those movements which are most rarely made. And, in order to measure the development of this phase of fatigue it is necessary to select some form of motor activity in which we can compare the strength and magnitude of the controlled impulses with those which escape control and cause the contraction of muscles which the subject is trying to inhibit.

An experiment which embodies both of these conditions was a modified form of the ergograph experiment. Instead of binding down all the fingers of the hand on which the experiment is made except the middle finger which is used in lifting the weight of the ergograph, the first and third fingers were each attached to a spring in the same way that the middle finger was attached to the weight of the ergograph. The tension of these springs was such that a stretch in either of them of one centimeter was equivalent to a lift of six and one half pounds. There was a set screw attached to each spring so that the springs could be adjusted to any tension desired. In the experiments the springs were each "set" with a stretch of six millimeters, so that any further stretch such as would result from the slightest bending of either the index or third finger would represent a lift by this finger of slightly more than four pounds, and a stretch of four millimeters in each of the two springs or eight millimeters in the two springs combined, in addition to the tension at which they are set, would represent a lift (by the two fingers combined) of thirteen pounds, the same as the weight used on the ergograph. (The weight used on the ergograph was six kilograms or thirteen and two tenths pounds.)

To each of the wires by which the springs were attached to the fingers, a stylus was fastened in the same way that the stylus was fastened to the wire that raised the weight of the ergograph, and in such a manner as to make tracings parallel to that made by the latter stylus on the smoked paper of the kymograph drum. In this manner I was able to compare the strength of uncontrolled impulses, as shown by the involuntary contraction of the muscles which flex the first and third fingers, with the strength of the controlled impulses as shown by the extent to which the middle finger could be bent. (See Figs. 1 to 4, for sample tracings.) The lower curve was made by the middle

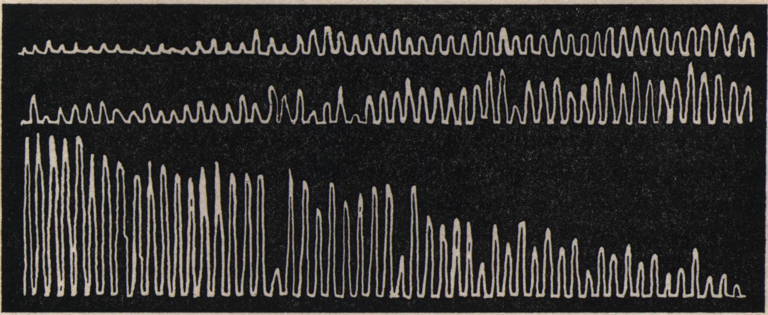

FIG. 1. (Reduced one-half.) Lower curve shows the successive flexings of the middle finger; second curve, those of the third finger; upper curve, those of the index finger.

finger while lifting the weight of the ergograph; the middle curve by the index finger; and the upper one by the third finger.

In all the experiments, the hand, wrist and forearm were rigidly bound to the board on the ergograph on which they rested so that they could not be moved in any way whatever. In addition to this, the usual method of binding the hand and arm in experiments with the ergograph, two pieces of cork about one and a fourth inches in diameter and an inch thick were placed on the palm of the hand, one over the knuckle joint of the index finger and the other over that of the third finger, and a strip of canvas was wrapped tightly over these pieces of cork and around the board on which the back of the hand rested and securely fastened with a clamp, so that there could be no bending of these fingers except in the first and second joints from the finger tips. The rhythm at which the weight was lifted was regulated by a metronome set to beat seconds. The weight was lifted at one beat and allowed to drop back at the next, making one lift for every two seconds.

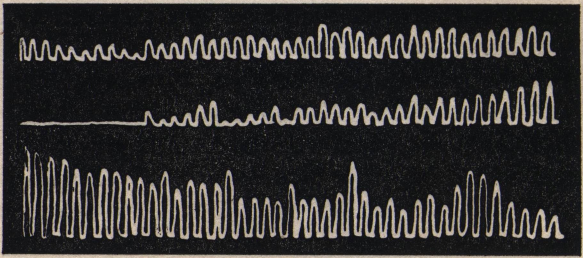

FIG. 2. (Reduced one-half.) Lower curve shows successive flexings of the middle finger; second curve, those of third finger; upper curve, those of index finger.

The subject was instructed to lift the weight as high as possible each time, but was to try particularly all the time to prevent the bending of either the index or the third finger.

In some of the experiments there was a slight bending of the index and third fingers, especially the latter, almost at the beginning while in others it was delayed for a considerable time. The stage in the experiment at which the index and third fingers began to bend perceptibly depended very largely if not entirely on the amount and kind of training the subject had had in the use of the hand and the fingers individually.

Fig. 1 is the fatigue curve of a subject who had done very little muscular work of any kind for a number of years. In his case he was unable entirely to inhibit the action of the first and third fingers at the very beginning of the experiment although the bending then was not nearly so great as at the close. Figs. 2 and 3 were made by subjects who operate a typewriter quite extensively but who use only the first and second fingers in striking the keys. They had thus developed the ability to use these two fingers independently of each other quite freely; but never having used the third independently of the others in any kind of work, they were unable to inhibit its action as they were that of the first finger. Fig. 4 was made by

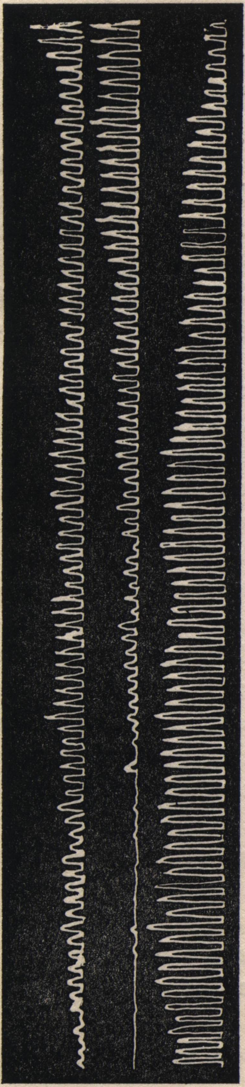

FIG. 3. (Reduced one-half.)

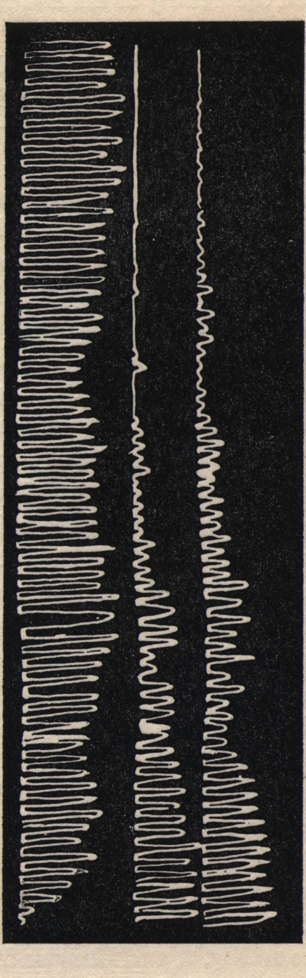

FIG. 4. (Reduced one-half.) Lower curve shows successive flexings of middle finger; second curve, those of third finger; upper curve, those of index finger.

a subject who was working several hours daily in the machine shop and other manual training shops of the university and had developed a great deal of skill in the use of his hands.

In order to express numerically the relation between the strength of the uncontrolled or involuntary impulses which caused the flexing of the first and third fingers with that of the voluntary impulses which caused the flexing of the middle finger, I calculated the number of foot-pounds of work accomplished by each finger in each successive ten second period during the experiment.

This was done for the middle finger by taking the sum of the distances through which the weight was lifted in five successive lifts, expressed in the fraction of a foot, and multiplying it by the number of pounds in the weight used on the ergograph (13.2 lbs.). To calculate the work done by the other two fingers I measured the amount of stretch in each spring for each five successive efforts, expressed in millimeters. But, since the work with these fingers was done on a spring, each increment of stretch in the spring represented the addition of a corresponding increment in the weight lifted. Hence, to calculate in terms of foot-pounds the amount of work done by these fingers, I took the maximum stretch of the spring at each effort and found the number of pounds lift which that represented. Then taking 3.9 pounds, the tension at which the spring was set, as the first term in an arithmetical series and the amount of lift represented by the maximum stretch as the last term and the number of millimeters stretch as the sum of the terms, I found the sum of the series by the formula: $S = (a + 1)n/2$. S represented in this case the number of millimeter-pounds of work done at each effort. I then added each successive five results and reduced the sum to foot-pounds by multiplying it by .00328, the equivalent of one millimeter expressed as a fraction of a foot.

The results are shown in the following tables. Column I. gives the foot-pounds of work done by the index or first finger, Column II. by the middle finger, Column III., the third finger, and Column IV. the amount done by the first and third fingers combined.

Table I. represents the same experiment as shown in Fig. 1, Table II. the same as Fig. 2, and likewise for Tables III. and IV.

To show that this loss of control over motor impulses developed before the muscles which flexed the middle finger were entirely exhausted or before the resistance in the motor tracts leading to these muscles had developed to a point which the organism could not overcome, I made another set of experiments slightly different from the first. In these experiments instead of attaching the first and third

TABLE I

I	II	III	IV
.47	9.35	.28	.75
.47	7.91	.20	.67
.49	7.43	.26	.75
.87	6.25	.43	1.30
.75	6.18	.71	1.46
1.27	5.36	.65	1.92
1.39	3.78	.76	2.15
1.64	3.37	.71	2.35
1.56	2.54	.83	2.39
1.94	2.06	.92	2.86
1.39	.62	.74	2.13

Note.—The last period represented only eight seconds' work or four lifts.

TABLE II

I	II	III	IV
.00	4.88	.47	.47
.00	3.99	.35	.35
.25	3.23	.62	.87
.24	2.82	.59	.83
.37	2.21	.72	1.09
.42	2.61	.74	1.16
.66	2.20	.88	1.54
.69	1.50	.95	1.64

TABLE III

I	II	III	IV
.00	2.81	.06	.06
.00	3.23	.21	.21
.01	3.92	.49	.50
.00	3.71	.46	.46
.05	3.51	.52	.57
.06	3.58	.65	.71
.24	3.58	.85	1.09
.40	3.16	.62	1.02
.27	3.30	.65	.92
.54	3.44	.79	1.33
.57	2.90	.72	1.29
.60	2.41	.59	1.19
.65	2.06	.56	1.21
1.05	2.68	.56	1.61
1.08	2.13	.59	1.67
1.32	2.03	.76	2.08
.90	1.51	.79	1.69

Note.—Last period only eight seconds.

TABLE IV

I	II	III	IV
.00	5.30	.00	.00
.00	5.50	.03	.03
.00	5.30	.11	.11
.01	4.81	.29	.30
.07	4.12	.17	.24
.07	3.78	.36	.43
.21	4.54	.52	.73
.29	4.88	.70	.99
.92	5.15	1.20	1.92
.53	4.40	1.01	1.54
1.19	3.85	1.01	1.20
1.79	4.33	2.23	4.12
2.11	1.36	1.82	3.93

Note.—Last period only eight seconds.

fingers to a spring they were held down by a metal clamp as is usually done in experiments with the ergograph. But when the middle finger had reached the limit of its capacity to raise the weight of the ergograph, the clamp that held the index finger was released, the subject continuing his efforts to raise the weight without any interruption in the rhythm. As soon as this was done the middle finger was again able to raise the weight; in some cases as high or even higher than at first and to continue raising it as long or longer than before the index finger was released. After a time, however, fatigue developed to such a point that the subject was no longer able to raise the weight. The clamp was then removed from the third finger, and the ability to raise the weight was immediately regained and continued for some time. (See Figs. 5 to 8 for results of this type of experiment. Points marked with a cross, X, indicate where the first and third fingers respectively were released.)

The same precautions were taken in these as in the former experiments to prevent any bending of the hand at the knuckle. The pieces of cork were set farther out on the first and third fingers, and they were bound down so that they lay flat on the board of the ergograph out to the second joint from the finger tips. The weight used on the ergograph in these experiments was eight kilograms instead of six as in the former ones. This added weight and the very tight binding of the hand made the records much shorter than in the previous ones.

As in the previous group of experiments so here we have the records of subjects of varying kinds and amounts of physical training. Figs. 5 and 6 are the records of the same subjects as are Figs.

2 and 3 in the preceding group of experiments. The fact that they had been accustomed to use the index and third fingers independently of each other and especially of the third finger would make it possible to direct the impulses into the muscles which flexed these fingers without innervating those which flex the third finger longer than would be the case if they had not developed an independent action of the different fingers. They would, therefore, be able to exercise the middle finger more nearly to the point of absolute exhaustion regardless of the action of the other fingers, and the binding of the

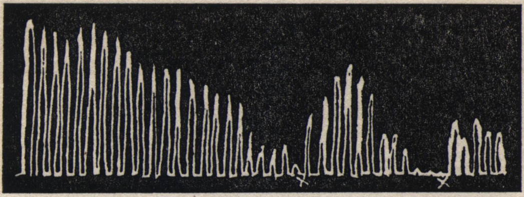

FIG. 5. (Reduced one-half.)

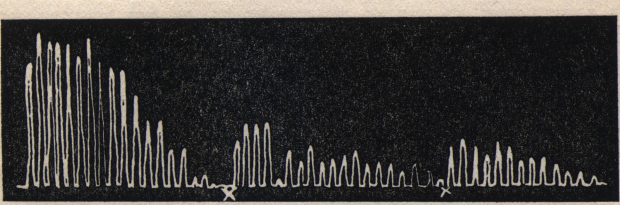

FIG. 6. (Reduced one-half.)

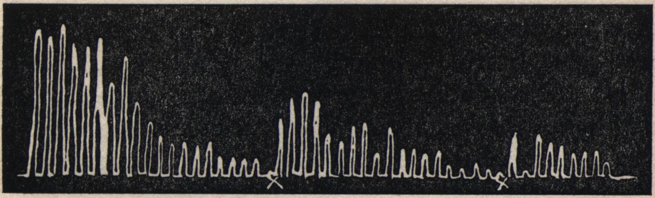

FIG. 7. (Reduced one-half.)

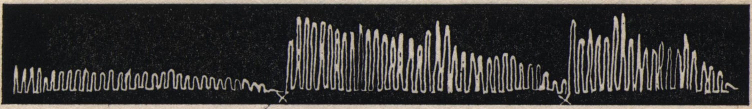

FIG. 8. (Reduced one-half.) Curve showing results of releasing index and third fingers while exercising the middle finger by raising the weight of the ergograph. Points marked with a cross indicate the points where the index and third fingers, respectively, were released.

other fingers would interfere less with its action, and hence, to release them would afford a smaller measure of relief than if each finger had not been trained to act separately. Fig. 7 is likewise the record of a subject who operated a typewriter quite extensively.

Fig. 8 is the record of a subject who had never in his life engaged in manual exercise to any great extent. His grip was strong, and in such an exercise as pressing the dynamometer he could exert a greater pressure than either of the other subjects. But because of lack of training he was unable to exercise one finger independently of the others.

Taking these two groups of experiments together, they seem to the writer to show quite conclusively that fatigue can not be explained entirely as due to a depletion of organic energies or to the increased resistance to motor impulses or to an exhaustion of the muscle tissues, nor even to all three causes combined. If fatigue represented simply a depletion of organic energies, where were generated the impulses that caused the flexing of the first and third fingers in the first group of experiments? If it represented only an increased ratio between available kinetic energy and the resistance which this energy must overcome in order to function in muscular contraction, how can we explain the regained efficiency in the middle finger when the first and third were released as shown in the second group of experiments? The second group of experiments also shows that muscular exhaustion is not all there is to fatigue.

The first group of experiments showed that there was still sufficient energy available for the middle finger to do the work required of it, since as soon as its efficiency began to wane, or even before, the subject began to lose control over his motor impulses and they began to flow out into the muscles which flexed the other fingers. It is true that this phenomenon could be explained entirely by an increased resistance in the motor pathways to the muscles of the middle finger. The second group of experiments showed that this resistance had not developed to a point which could not be overcome, since as soon as conditions were changed elsewhere so as to give the subject a greater freedom, the capacity of the middle finger (whose condition remained entirely unchanged) to do the work required of it was immediately regained.

We have previously stated that the first effects of fatigue are a disorganization, or an interruption of the function of control. We have also shown that there are other influences which produce like effects. One of these is a binding or hampering of any part of the body. This can be shown by such an experiment as pressing a dynamometer while standing with the legs in a strained or hampered position. While the muscles of the lower limbs are not involved in pressing the instrument, yet the fact that they are hampered makes it impossible to direct the motor impulses to the muscles of the arm with as great a force as if the lower limbs were free.

So here in the last group of experiments with the ergograph the disorganizing effects of binding the two fingers combined with those resulting from the fatigue of the middle finger were sufficient to destroy the control over motor impulses, while the subject could yet combat successfully the effects of either influence alone.

To sum up our results in a single sentence: While fatigue may be analyzed into such definite constituent elements as depletion of nervous energy, increased nervous resistance to motor or other nervous impulse, a lowered tone of the muscles themselves, due to toxic poisoning and other causes, and many other factors which may be determined as playing a part in its development, its first symptom will be an interference with the function of control over all kinds of motor expression; and it is this phase of fatigue which, in an organism all of whose members are intact, affords the best and most reliable index for measuring the development of normal fatigue.

PART II. MENTAL FATIGUE

I. THE PROBLEM OF MENTAL FATIGUE

COMING now to the main topic of our study, the part which deals with the question of mental fatigue, we find ourselves confronted, if not by an entirely different kind of problem, at least by one different in many of its most important phases, and requiring very different methods for its solution. In muscular activity it is possible, as we have seen, to study processes as such and to analyze fatigue into its various constituent elements. We can take a muscle or set of muscles either within the body with the circulation and nervous supply intact or removed from the body and by applying a series of artificial stimuli either to the muscle direct or through the nerve, we can observe just how the muscles respond to these stimuli. Or we can take a muscle that is still under voluntary control and by placing restrictions to the actions of contiguous muscles, and by subjecting it to a series of voluntary stimulations study the changes that take place in the manner in which it responds to these stimulations.

It is clearly evident that none of these methods will apply in a study of mental activity or in an analysis of mental fatigue. While it is generally assumed that all mental processes involve some kind of cerebral activity, yet the exact character of that activity is not definitely known. We can not select a particular center or group of centers within the brain together with the nervous approach to it, sever its connection with other contiguous or allied centers or place artificial restraints to prevent their functioning, and thus by subjecting it to a definite series of stimulations, study the changes which take place in its manner of functioning when thus subjected to a definite course of activity. The only restrictions or limitations that can be imposed to prevent the free response of a part of the mental faculties while others remain alert are such as can be determined by the character of the work required; and the only changes that can be studied in the character of mental processes which come as a result of mental exertion are such as are apparent in or can be inferred from the character of the work performed.

The problem, then, of estimating mental fatigue will consist first in determining some uniform change in the character of mental processes which result from mental exertion and then applying some form of test, the efficiency in performing which, will show the extent to which these changes are operative. But as we have seen in the study of muscular activity, efficiency, if measured solely by the gross results of the efforts expended, will continue unimpaired long after fatigue, the immediate effects of physical exertion, has modified the processes by which these results are secured. The oarsman rows the first few strokes with his arm and shoulder muscles, but in a short time his whole muscular system is brought into action. And yet, there is no apparent difference in the effectiveness of the first and last strokes. The tired pedestrian must "walk all over" at the close of a day's journey, in order to maintain the rate of speed at which he started in the morning. If we attempted to estimate the fatigue of either of these subjects solely on the basis of what he was able to accomplish we should find it no greater at the close of the exercise than at its beginning. And yet a study of all the factors involved in muscular exertion will show that fatigue has been developing during the entire exercise.

Whether or not mental processes and the performance of mental functions are in all respects analogous to physical processes and the performance of muscular functions may not be definitely determined; but it is definitely known that there are a large number of factors which influence mental efficiency. It will not do, therefore, to conclude that one is not fatigued after a period of mental work simply because he is accomplishing as much or more than at its beginning.

Another difficulty encountered in attempting to estimate mental fatigue entirely on the basis of its effects on general mental efficiency is in the fact that there are no units of value for measuring mental work. We do not know whether a greater amount of work is involved in solving a problem in algebra or in committing to memory a stanza of poetry. Nor can we tell, objectively, whether it requires more work to recall a proposition in geometry or a date in history. But since muscular fatigue did not at first express itself in a decrease in the units of work performed, perhaps we should seek elsewhere than in the capacity to perform the ordinary mental functions for the first expressions of mental fatigue.

If, as is usually assumed, mental processes involve some form of cerebral activity, and if our efforts to make mental associations and to interpret and elaborate our sense impressions cause the release or discharge of nervous energy within the nervous centers which gives rise to impulses that pass from one center to another, and if the direction which these impulses take determines the kind of associations we shall make and the interpretations which we shall give to sense impressions and the attitudes we shall take toward them, then it must be possible for these impulses or excitations to take different directions, or our mental response to every situation would always be the same. But since they are not always the same and because we are able to determine very largely what kind of associations we shall make then it follows that there must be some means by which we are able to control the direction or select the pathways which these nervous excitations shall take.

We found in our study of muscular fatigue that the first and most constant results of physical exertion was a loss of control over the direction which motor impulses shall take. It seems reasonable to assume that a similar result would follow mental exertion. If this hypothesis be true, then our first problem in devising a scale for measuring mental fatigue will be one of finding some form of mental exercise or mental test, the performance of which will show most clearly the degree of control which the subject can exercise over the direction which nervous excitation within the brain shall take.

A form of test based on this conception of the character and the conditions of mental fatigue, and which takes into account the other conditions that influence the results of mental effort should possess at least three distinct features:

1. The processes tested should be identical at each separate application of the test. Otherwise we should have no means for comparing the results of different tests. Since we have no units for measuring mental work, the only relation which establishes an objective comparison between two mental processes is the relation of exact similarity, or what we may term *"generic identity."*

2. The variables arising in performing the process tested should be limited to one type and should be such as can be expressed in units that are susceptible of mathematical treatment. If more than one class of variables arise in performing the test, there is a possibility that the results indicated by one variable may be offset by those shown by the other. This is particularly true of those tests which attempt to measure both speed and accuracy in mental operations. If the variables can not be treated mathematically, then all comparisons become simple approximations and the verification of results is not possible.

3. The function tested should be one that will be interfered with rather than aided by the exercise of other functions. Just what is meant by this statement will be made clear when the specific tests used are explained.

II. PRELIMINARY ATTEMPTS IN EXPERIMENTS IN MENTAL FATIGUE[1]

Three different types of experiments were tried in the attempt to test the principle that we have been discussing before any satisfactory results were secured. All were abandoned, however, either because the results secured by them seemed to be too much affected by other factors, or because the data secured seemed too vague and indefinite.

The first experiment consisted in trying to do some special kind of work in the midst of distractions, and estimating the results by the speed and accuracy with which the work was done. This was soon abandoned, since it involved too many indeterminate factors. It was not possible to maintain the distractions as a constant quantity. The work varied in difficulty and the amount of concentration required. There was always a question as to the relative values given to speed and accuracy in the results.

The second experiment consisted in an attempt to measure the success with which one can carry on two different lines of mental activity at the same time, such as reading or committing while counting the ticks of a clock. Here again it was found impossible to evaluate results with any degree of assurance. The speed in reading, committing or performing numerical computations varies when done as a single exercise. The problem of checking and scoring errors offered another difficulty. The assumption in the first tests was that there would be a sort of contest between the efforts of the subject to concentrate attention and the distractions which as sensory stimuli would tend to draw attention away from the work. The degree of control would be indicated by the subject's success in combatting these distractions. In the second it was thought that a greater degree of control would be required to carry on two lines of activity than one, and hence would be more disturbed by fatigue.

The third form of experiment tried was suggested by accident, and it was the attempt to reduce it to a usable form out of which the form of experiment that was adopted was developed. In the room in which this study was begun and in which the principles described first began to take form in my mind was a picture, about 20 × 30 inches, of a girl sleeping on the ground with her head resting against the body of a large shaggy dog. This picture hung in such a relation to the window and the table at which I usually worked that when seen from the table, a distance of about twelve feet, the real picture could not be distinguished but instead it seemed to represent a forest scene with a background of billowy clouds. I observed at different times that if I looked closely at the picture and got all its outlines well fixed in mind, and then while still looking at it walked to the table and sat down, I could still hold these outlines and see the picture as it appeared when viewed closely. On the other hand, if I first looked at the picture from the table without approaching closely enough to see its details, I could not recall the exact appearance of the picture or trace any of its outlines. In the evening, however, after a day of reading and study I found it difficult, and, sometimes impossible to hold in mind the picture as it appeared when viewed closely only so long as I could actually see its most important outlines. As I interpreted it, this effort to keep all the details of the nearer view of the picture vividly before me while actually seeing it as it appeared from a distance represented a conflict between two tendencies, or rather between volition and a natural tendency. My efforts were to disregard the actual visual impression and to cling to the unconfused concept of what I knew to be there. On the other hand, the natural tendency would be for the retinal image of the picture as it appeared from the distance to determine the character of the mental image or concept which I should have of it.

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[1] No review or resumé of the various methods that have been employed in measuring mental fatigue will be here given. To do so would simply be to repeat what has been systematically reviewed in a number of works on fatigue. Moreover, since this study attempts to approach the subject from a different angle from that from which others have treated it, their methods, however reliable, will afford little aid in solving the problem as we have stated it.

The principle involved was clearly one of control. To see the picture as a representation of a girl and a dog involved the activity of certain brain centers; while to see it as a forest scene involved the activity of others. The activity would take place in those centers which were reached by the nervous excitation caused by the visual stimulus. Usually the character of the visual stimulus will determine the course which this excitation will take. But when rested and with no other conditions present to interfere, I was able to direct the nervous excitation in such a way as to leave those centers dormant which would be involved in seeing the picture as it usually appeared from a distance; or if they were not actually dormant their activity was less vigorous than that of those involved in seeing it from the nearer view point. Fatigue, however, destroyed this control and the excitation followed its normal course.

While this experience (I should hardly call it an experiment), seemed to be in accord with the theory I had worked out, yet, as a practical usable experiment for laboratory use in measuring mental fatigue, it did not seem applicable. In the first place the appearance of the picture from the more distant viewpoint would vary according to the way the lights and shadows would fall upon it.

Then the results of different applications of the test were not susceptible of mathematical comparison. I was either able to hold in consciousness the image of the picture as it appeared when viewed closely while looking at it from a distance or I was not. There seemed to be no intermediate ground, no means by which varying degrees of fatigue would be indicated. While I could feel subjectively that a greater effort was required sometimes to hold the image of the real picture in consciousness when looking at it from a distance than at other times; and while it was sometimes necessary to walk up to the picture two or three times in order to fix the near appearance sufficiently vivid as to make it remain after walking away, yet these factors were not of such a character that they could be given a definite value.

While seeking to modify this test in some way so as to avoid the difficulties involved in the form in which I had been trying it, it occurred to me that the same underlying principles were involved in the process of reversing the perspective of an ambiguous figure, especially if the reversals are voluntary or forced. It would also be possible, with this sort of test, to compare the results obtained from different applications of it.

We may define the reversal of the perspective of an ambiguous figure as a process by which the spatial relations of the lines, angles and shadows of a drawing or other representation of an object, are mentally exchanged or interchanged in such a manner as to give a mental concept of the object either in a changed position, or with the relations of certain of its parts reversed, or with our viewpoint with respect to the object changed.

An examination in detail of this process of reversing an ambiguous figure will show that it not only involves in a very high degree the principle of control in the exercise of mental functions, but also satisfies the other conditions which we have said should characterize a scale for estimating mental fatigue. 1. As we have previously stated, in order to compare the results of different applications of the test the process tested must be identical for each separate application. There can be no doubt that the processes involved in making one reversal will be identical both in kind and magnitude with those involved in making every other reversal. 2. Because the reversals do not vary in kind or magnitude, the only variable arising in making the reversals is the element of time. If we analyze these reversals we shall find that they involve certain definite steps. The visual image of the drawing revives in our minds a clear concept of the real object represented in a definite position. The principle is the same as that involved when we look at the photograph of a familiar face. What we actually see is not the photograph with its particular arrangement of lines and angles, and of light and shade. The image or concept which we have while looking at the photograph is distinctly that of the individual himself. The same is true in any sort of representation. Its purpose is to suggest, to revive the mental image or concept of the thing represented.

After getting our concept of the object in one position or from one viewpoint, the first step in making the reversal will be to recall the appearance of the object as it would be in a reversed position or if we were looking at it from another point of view, and then, while holding this latter concept in the focus of consciousness, make the mental exchange of the spatial relations of lines and other details of the representation so that our visual impression coincides with, or as we sometimes say, ''squares'' with our mental concept.

We saw in our experiments with the ergograph that the loss of control resulting from muscular exertion was first shown in connection with those movements which are most unusual; or still earlier in such forms of voluntary action as are in direct opposition to the organ's usual or habitual manner of acting. To flex the middle finger without flexing the others is not only unusual but contrary to ordinary practice. The bending of one finger is much easier when the others can bend with it, even though they can give no aid in lifting its load. When the capacity of the middle finger to lift its load while acting alone was lost through continued activity, it could in a measure be restored by permitting the other fingers to act, but only when their action was of the customary or habitual type. It is more difficult to control the direction of a part of our motor impulses while inhibiting others than if the latter are allowed to take their normal course. But we can perform more difficult feats when rested than when tired.

The same is true with respect to the different forms of mental activity. Perception will be aided and supplemented by inference but only when that which is inferred is in accord with what is perceived. For example, I can understand much more easily the relation of the parts in a piece of machinery if I can infer their use. But this inferred use must be in accord with what I have already perceived in this relationship. Otherwise, it will be confusing instead of illuminating. Mental imagery is strengthened by sensory impressions; but only when the two are alike in character. It is very difficult to image the feel of flour or talcum powder, if one has his hands in sand. It is almost if not quite impossible for one to conceive all the feelings of frosted ears or fingers while sweltering in a temperature of 98° or above.

3. Applying these principles to the processes involved in the reversible perspective we can see that it satisfies the third requisite of a scale for measuring mental fatigue. The process of making the reversals will not be aided by the exercise of the usual functions or by the application of the usual methods by which we read meaning into special situations. Our first step in interpreting any situation is to grasp all the details as they are presented to us through our senses and then by applying our past experience read into it whatever meaning seems to fit these details. In reversing the perspective of an ambiguous figure the process is just the reverse. Instead of having the mental concept determined by the sense impression we modify the latter by the former. All our mental attitudes result either from impressions (which come to us either as direct representations or as suggestions); or from our own volitions. Effort or suggestion can, however, only recall past impressions; they can not create impressions *de novo.* If by effort I can recall only faintly any impression or concept, I can supplement volition and make the impression more vivid or the concept clearer by repeating the experience which first gave it.

If the mental concept desired is that of an object as it is represented by one of the perspectives of a reversible figure it can be made clearer by seeing the figure with that perspective but not by seeing it with the other perspective. Thus to reverse the perspective of an ambiguous figure while looking at the figure is in direct opposition to the usual method of strengthening impressions by repeating the experience which first gave them.

If mental concepts can result either from stimuli playing upon the sense organs or from the organism's own volitions, then it must follow that either of these agencies can cause the excitation within the brain centers which produce these concepts. It is likewise evident that the excitation will be greater and the concept correspondingly clearer if the two agencies which produce them are acting together than if they were acting in opposition to each other, and the results will be secured more quickly. And as a corollary to this latter proposition, it is equally evident that mental concepts which result from volition working in opposition to sensory stimuli will be recalled or produced with greater difficulty and hence more slowly after fatigue has interfered with and impaired the function of control over the direction which nervous excitation shall take.

The capacity to receive sense impressions through the sense organs will continue long after fatigue has made it difficult or even impossible to organize or relate them to a particular past experience. The artist or naturalist does not cease his observations or his explorations because he can no longer see the objects which come within his range of vision, but because he can not correct and modify his first impressions and make them coordinate with all his past experiences with the same or similar situations. That which to a tired scientist or naturalist is only a rough boulder may have for him when rested a hundred meanings because he will then be able to modify and correct every false impression which his sense organs first convey and to read all the possible meanings into every true impression.

The same principle is, as I believe, involved in the process of reversing the perspective of an ambiguous figure. When rested and with the faculty of control unimpaired, it will be comparatively easy to modify visual impressions made by the lines and shadows of the figure and to relate them to any of our mental concepts between which and these impressions there is a relation either of similarity or identity. But as fatigue develops, this ability will be lessened and the process of making the reversals will become more difficult and therefore slower.

This method of measuring fatigue does not attempt to measure in any inclusive or comprehensive manner the gross capacity of the psychical side of the organism to perform its ordinary functions. What it does seek to discover are the effects of mental exertion upon one specific mental function; but since this function is one most susceptible to the influences of fatigue the test should, indirectly, give a reliable index of the fatigue state. In our experiments with the ergograph we have found that muscular fatigue did not first express itself in an inability to make muscular contractions or execute movements, but in the inability to control and coordinate efforts in executing a particular kind of movement. So here in the experiments with the reversible perspective we are attempting to measure not the ability of the mind to receive and interpret ordinary sense impressions or exercise its other general functions, but the ability to correct and, in a sense, to control sense impressions in such a manner as to make them produce a specific mental attitude. Or we may express the analogy in another way: The experiments with the ergograph did not seek to determine the amount of available physical energy at the disposal of the organism. What they did attempt to measure was the *ability* of the organism to *direct* the discharge of that energy in any desired channel and produce a specific form of action. Nor do the experiments with the reversible perspective attempt to measure the amount of mental or psychic energy available for the performance of the

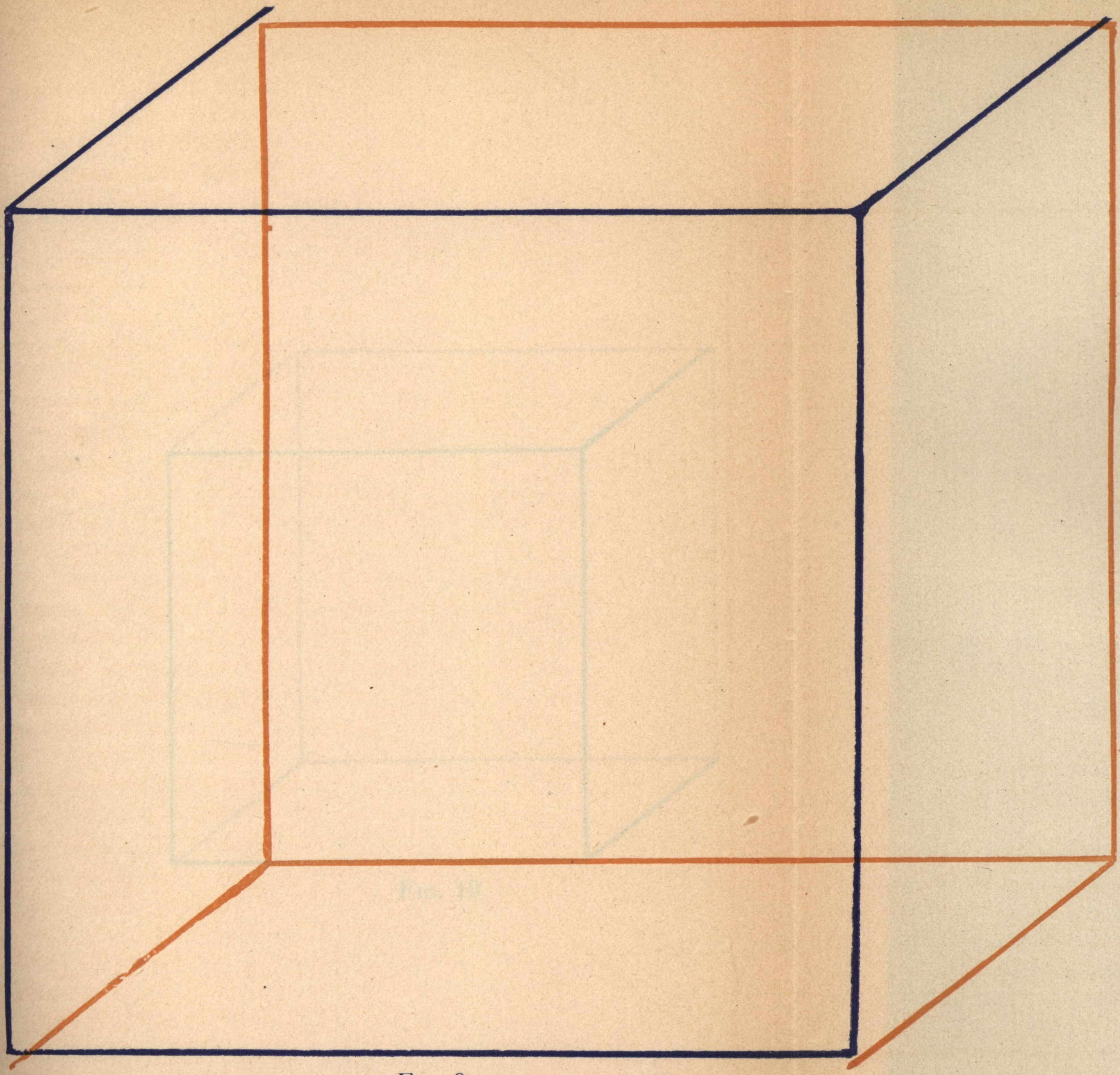


Fig. 9

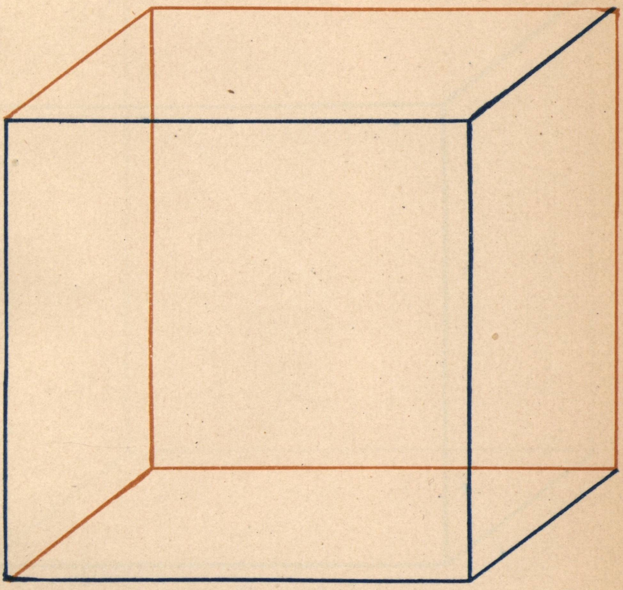


FIG. 10

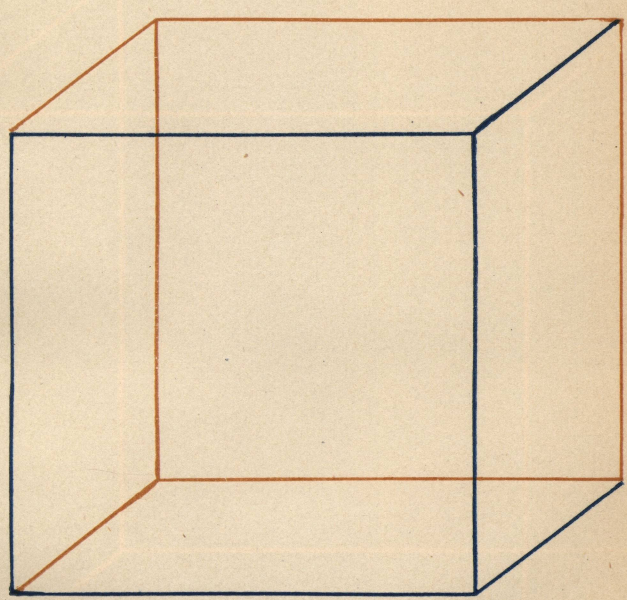


FIG. 10

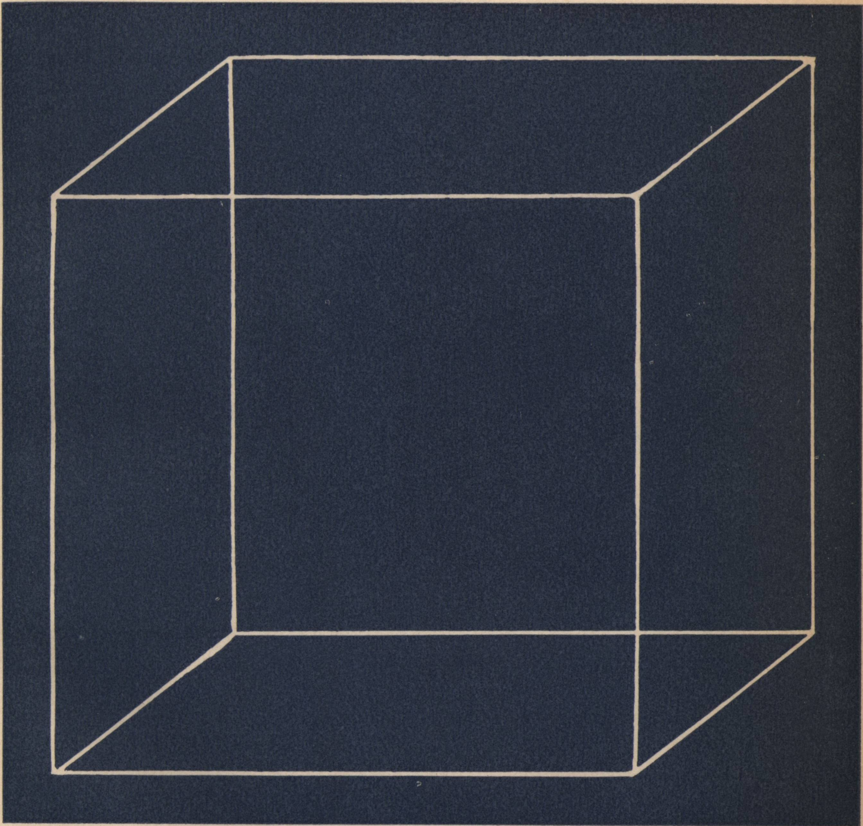
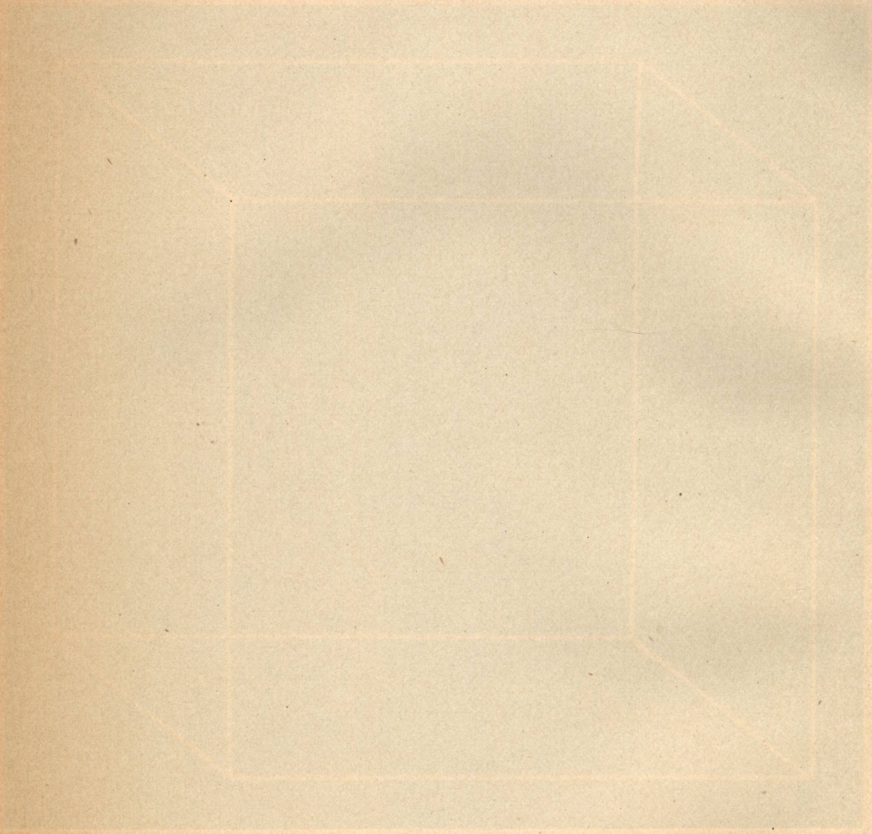


Fig. 11



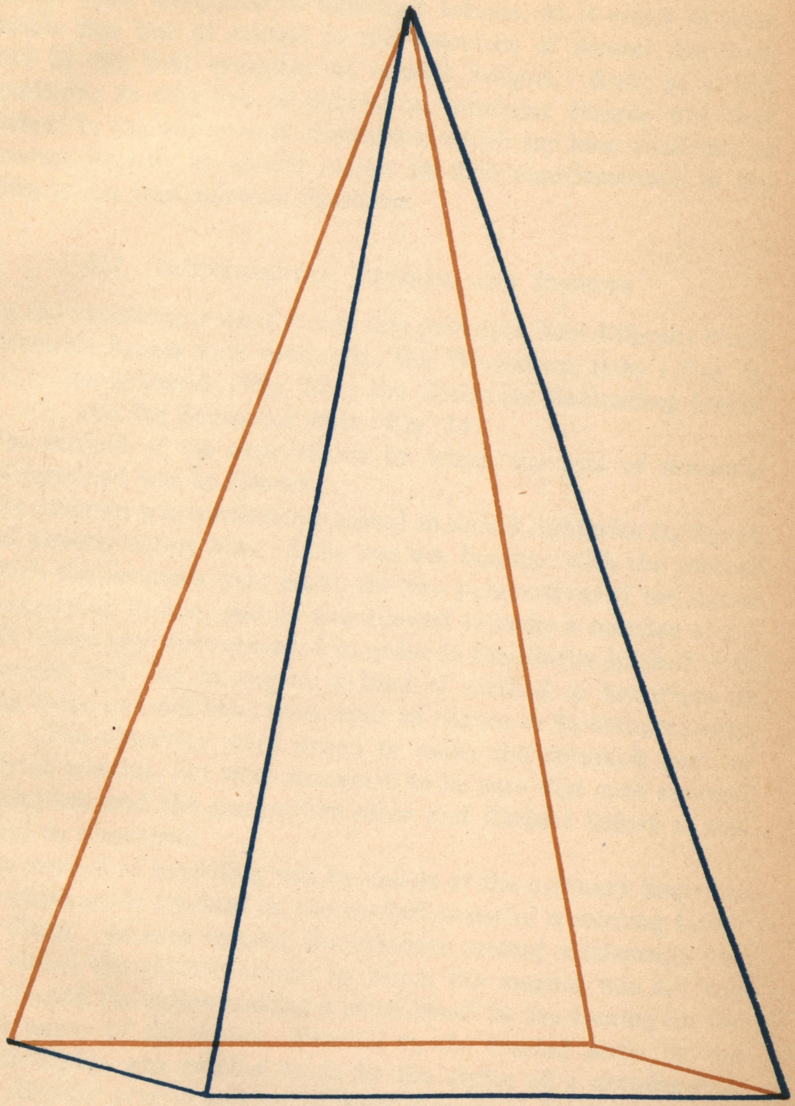


FIG. 12

ordinary mental functions, but rather the ability to control the disposal of that energy in the performance of a very specific function. And, just as we found that loss of control in muscular activity precedes the other symptoms of muscular fatigue, so it seems at least plausible that loss of control in the execution of mental functions should be the first symptom of mental fatigue. And, as a last proposition; as this loss of control in muscular fatigue was first expressed in connection with movements which are least common; so in mental fatigue, we should expect its first manifestations in the exercise of the least common functions.

III. Experimental Methods and Results

In the experiments used to test this principle four different kinds of reversible figures were used: viz., the Wheatstone cube (Figs. 9, 10, 11); the pyramid (Fig. 12); the Scripture fluctuating blocks (Fig. 13), and the Schroeder stair (Fig. 14).

The method of the experiments by which the rate of reversals was determined was as follows:

The observer was comfortably seated in a good light with the figure placed directly before him. If he was not familiar with the process by which the reversals were made, the two perspectives of the figure were explained to him, and he was allowed to make a number of reversals before any were recorded in order to familiarize himself with the process and also to acquire a kind of method or technique in making them, and not have them come as chance or fortuitous occurrences. The observers were urged to make the reversals just as rapidly as possible, but were cautioned to be sure that each reversal was complete and the perspective clear and distinct before it was recorded as a reversal.

The method of recording was by means of the ordinary magnetic stylus which made tracings on the smoked paper of a rotating kymograph drum. At each reversal the observer pressed a telegraph key which closed the electric circuit in which the magnet was set and thus deflected the stylus making a short break in the tracing on the smoked paper of the drum. Parallel to the tracing made by the magnetic stylus was another made by the stylus of a chronograph placed directly over it and set to mark seconds in its tracings. In this way two parallel tracings were made simultaneously, one of them marking seconds and the other the reversals. The average "reversal time" was found by dividing the number of seconds through which the experiment was continued by the number of reversals made.

In the first group of experiments, the results of which are shown in Table V., the first tests were made immediately preceding a period of one hour's very intensive mental work and the second directly following it. The work consisted of a form of mental addition devised by Professor Starch and published in his "Experiments in Educational Psychology" (27). The work was done in accordance with the published instructions which are about as follows:

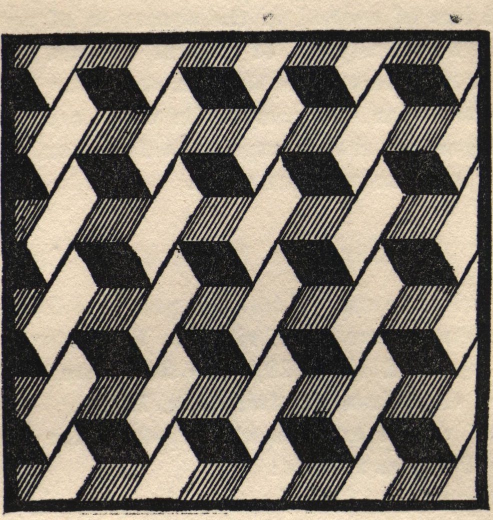


FIG. 13.

The observer was seated comfortably in a quiet room. The experi-

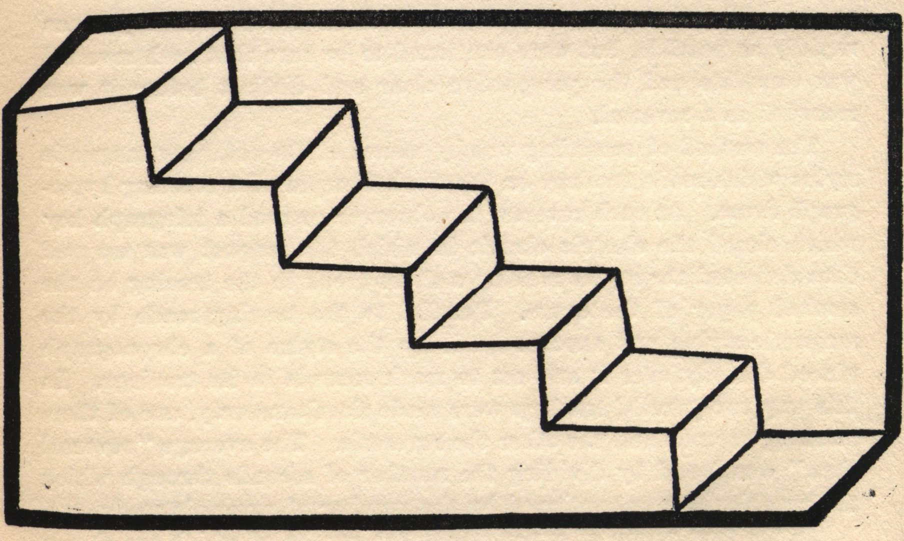


FIG. 14.

menter announced to him a number consisting of two digits. The observer then added six to this number, then seven to the new sum, then eight to that, and then nine, and then again six, seven, eight, and nine in rotation, etc. When the sum of successive additions had reached one hundred or more the extreme left-hand digit was dropped, and the addition continued with the two remaining digits. Thus, if the number given were 72, the consecutive sums would be 78, 85, 93, 102, 108, etc., but instead of giving these latter sums as 102, 108, 115, etc., they were given simply as 2, 8, 15, etc. A new number was announced by the experimenter every thirty seconds. In each case the observer added six, seven, eight, and nine in rotation to the sum gotten after each successive addition. In order to familiarize the observer with all the conditions of the work two or three preliminary trials were made, using numbers that did not occur among the numbers used in the regular work. The numbers used for "starters" comprise all the numbers between 10 and 100 except those whose right-hand digit is 0 or which will produce sums whose right-hand digit is 0. All these were omitted because the additions in such cases are decidedly less difficult.

After the preliminary trials were made and the observer understood thoroughly the character of the work he was asked to close his eyes and add just as rapidly as possible. He spoke his sums aloud. When a new number was given him he was to lose no time, but start at once and add at his maximum speed until another was announced. He was also to try to make his work as accurate as possible, but in case he became confused and forgot his last sum or the number to be added he was not to delay and attempt to recall it but to take the number which suggested itself as the probable one and go right on with the additions. The purpose was to make the work just as intensive as possible, and for this purpose speed was thought to be a primary requisite. The reversible figures used in this group of experiments were the cube with five inch sides (Fig. 9) and the pyyamid with a three inch base and an altitude of five inches (Fig. 12).

In Column I. of the table are given the observers' initials; in Column II. the date of the experiment; in Column III. the reversible figure used in the test; in Column IV. the average reversal time before the work period; in Column V. the average reversal time after the work period; and in Column VI. the percentage of increase in reversal time in the "after tests" over the "before tests." The sign (+) is inserted simply to indicate that in each case the time was longer after the period of work than before.

TABLE V

Observer	Date	Figure	Rev. Before (Sec.)	Rev. After (Sec.)	Per Cent. Increase
I. E. A.	Dec. 4, 1911	9 Cube	1.90	2.22	+ 17
I. E. A.	Dec. 4, 1911	12 Pyramid	1.46	1.88	+ 28
J. A. S.	Dec. 11, 1911	9 Cube	.79	1.36	+ 72
J. A. S.	Dec. 11, 1911	12 Pyramid	.90	1.30	+ 44
A. M. L.	Dec. 27, 1911	9 Cube	1.31	2.65	+ 102
A. M. L.	Dec. 27, 1911	12 Pyramid	.98	2.41	+ 146
W. A. C.	Dec. 28, 1911	9 Cube	2.25	4.00	+ 77
W. A. C.	Dec. 28, 1911	12 Pyramid	1.30	2.00	+ 54
J. W.	Dec. 29, 1911	9 Cube	1.08	2.00	+ 85
J. W.	Dec. 29, 1911	12 Pyramid	1.81	2.25	+ 24
I. E. A.	Jan. 3, 1912	9 Cube	1.63	2.12	+ 36
I. E. A.	Jan. 3, 1912	12 Pyramid	1.18	1.57	+ 41
J. A. S.	Jan. 10, 1912	9 Cube	.88	1.32	+ 50
D. S.	Jan. 13, 1912	9 Cube	1.36	1.80	+ 32
D. S.	Jan. 13, 1912	12 Pyramid	1.70	1.86	+ 9
V. A. C. H.	Jan. 19, 1912	9 Cube	1.90	2.32	+ 22
V. A. C. H.	Jan. 19, 1912	12 Pyramid	1.10	1.47	+ 34
O. C. T.	Jan. 20, 1912	9 Cube	1.88	2.37	+ 26
O. C. T.	Jan. 20, 1912	12 Pyramid	1.36	2.00	+ 47
J. A. S.	Jan. 29, 1912	9 Cube	.74	1.14	+ 54
J. A. S.	Jan. 29, 1912	12 Pyramid	.69	1.10	+ 59

The work in the experiments given in Table VI. was the same as that in Table V.; but was continued for one and one half hours instead of one hour only.

TABLE VI

Observer	Date	Figure	Rev. Before (Sec.)	Rev. After (Sec.)	Per Cent. Increase
I. E. A.	Jan. 8, 1912	9 Cube	1.71	2.59	+ 52
W. A. C.	Apr. 6, 1912	9 Cube	2.16	3.41	+ 58
O. C. T.	Mar. 30, 1912	9 Cube	.80	1.21	+ 51
O. C. T.[2]	Apr. 5, 1912	9 Cube	.71	1.45	+ 104

The foregoing tables show that the average time required to reverse the perspective of an ambiguous figure is decidedly longer after a period of intensive mental exertion than before, the increase in reversal time varying from 22 per cent. to 102 per cent. or an average of 52 per cent. for the cube in the one-hour experiments; and from 9 per cent. to 146 per cent. (average 48.6 per cent.) for the pyramid. In the experiments in which the work was continued for an hour and a half the increase in reversal time for the "after tests" over what it was for the "before tests" varied from 51 per cent. to 58 per cent. (average 53.6 per cent.).

[2] In the experiment of O. C. T., April 5, 1912, the work period was two hours instead of one and one half hours as in the other experiments in this table.

This may at first seem to indicate that the fatigue from one hour's work is just about as great as for one and a half hours' work. The reason for this apparent paradox is that we did not have all the observers for the latter group of experiments that we had in the former. The average increase in reversal time in the one hour experiments for those observers who did the one and one half hour experiments also was 39 per cent. as against an average of 53.6 per cent. for the experiment using the longer work period.

For the single observer, O. C. T. who did the one hour, the one and one half hour, and also the two hour experiments, the increase in reversal time was 26 per cent., 51 per cent., and 104 per cent. respectively in the "after tests" above what it was in the "before tests." Observer W. A. C. showed a greater increase in reversal time after one hour's work than after one and one half hours' work. This was probably due to the fact that this observer experienced considerable difficulty in acquiring any definite technique in making the reversals.

In order to vary the character of the experiments and to determine the fatigue effects of a different sort of mental work, I had several observers take the test in reversing the figure immediately following a two-hour semester-final examination (the examination was held from eight to ten o'clock in the forenoon), and a second test during the inter-semester vacation, from five to nine days after the first test. The purpose in having the two tests as far apart as they were was two-fold: first, to eliminate as far as possible from the second tests any effect of the practise in making the reversals which might have been acquired in the first tests; and, second, to give ample time for complete recovery from all worry and extra work incident to the final examination. The second test was taken at least three days after the last examination had been taken by any of the observers; while none of them had done any kind of work, either mental or physical, between their last examination and the time when the last test was taken. (The results of these experiments are shown in Table VII.)

The reversal time in the tests taken immediately after examination was in every case decidedly longer than in the tests taken during the vacation, the increase varying from 17 per cent. to 92 per cent. It is not at all probable, however, that the fatigue as shown by the difference in reversal time is entirely the result of the single examination, although it was the first examination of the day. Most of the observers had, however, been employing the time rather strenuously during the few days immediately preceding the examination in preparation for it and for their other examinations.

TABLE VII

Observer	Date	Figure	During Vacation (Sec.)	After Examination (Sec.)	Gain or Loss Per Cent.
B. F.	Feb. 1912	9 (Cube)	2.20	3.00	+ 27
A. M. L.	Feb. 1912	9 (Cube)	.55	.88	+ 17
D. G.	Feb. 1912	9 (Cube)	1.37	2.39	+ 74
L. G.	Feb. 1912	9 (Cube)	1.40	2.69	+ 92
H. S.	Feb. 1912	9 (Cube)	1.33	2.50	+ 88
J. W.	Feb. 1912	9 (Cube)	1.35	2.09	+ 47
F. H.	Feb. 1912	9 (Cube)	1.34	2.00	+ 49
E. C. E.	Feb. 1913	11 (Cube)	.52	.74	+ 42
E. C. E.	Feb. 1913	13 (Blocks)	1.33	1.83	+ 37½

Note.—Observer E. C. E. took the first test at nine o'clock in the morning and the second at noon. The intervening time was occupied in the preparation of a public address.

A comparison of the results of the first and second groups of experiments as shown in Table V. and Table VI. with the results of the third group as shown in Table VII. would seem to show that the immediate fatigue resulting from a two hour examination is not as great as that resulting from one hour's work of the sort done in the first and second group of experiments. This inference is in accord with the subjective estimates of the two observers, A. M. L. and J. W. For A. M. L. the average time required to reverse Fig. 9 was 102 per cent. greater after the one hour's addition than before, while the average reversal time immediately after the examination was only 17 per cent. greater than during the vacation, three days after engaging in any kind of mental work. This observer was not of a disposition to allow his examinations to burden him. He did his university work well and was sure of his grades. The additions, however, he found very difficult. With J. W. the difference was 85 per cent. between the reversal time in the "before" and "after" tests in Experiment I., Table V. The difference in Experiment III., Table VII. was 47 per cent. This observer had taken two examinations on the previous day, and had spent the three hours immediately preceding this examination in preparing for it.

We should naturally expect that the fatigue resulting from the additions would be very much greater than that from ordinary study or from writing an examination. The relation of the two kinds of work is somewhat similar to that between a sprint and ordinary manual labor. Fifteen minutes running even at a very moderate pace will exhaust a man accustomed to manual labor much more than a half day's hard work of the kind to which he is accustomed. All the observers declared the mental additions the most strenuous and taxing kind of mental work they had ever engaged in.

In the next group of experiments tests were made hourly by instructors in the department of education in the university while grading examination papers. But one of the observers in this group of experiments (D. S.) worked very steadily or very intensively. He began work shortly after 1:30 and worked practically without interruption, except for the few minutes occupied in making the tests, until near 6:00 o'clock. V. A. C. H. and G. F. W. worked rather leisurely and with frequent interruptions. The actual time spent in marking papers by V. A. C. H. did not exceed one and one-half hours, and the greater part of this was during the hour immediately preceding the last test. Tests were made approximately at 2:00, 3:00, 4:00, and 5:00 o'clock and by D. S. at 5:45. This was the first experience for G. F. W. and doubtless practise exerted more of an influence upon his results than upon the results of those observers who had already acquired a definite method.

The results of the experiments in Group IV. are shown in Table VIII.

TABLE VIII

Observer	Date	Figure	1st Test (Sec.)	2d Tcst (Sec.)	3d Test (Sec.)	4th Test (Sec.)	5th Test (Sec.)
D. S.	Feb. 3, '12	(9) Cube	1.67	1.86	2.51	3.05	3.86
V. A. C. H.	Feb. 6, '12	(9) Cube	1.54	1.51	1.58	1.78	—
G. F. W.	Jan. 30, '12	(9) Cube	.80	.97	1.30	1.01	—
G. F. W.	Jan. 30, '12	(12) Pyramid	.77	.91	.98	1.13	—

The numbers in the columns headed 1st Test, 2d Test, etc., indicate the reversal time at 2:00, 3:00, 4:00, 5:00, and 6:00 o'clock respectively.

A number of observers had expressed a feeling that perhaps the figures we had used in the preceding tests (Figs. 9 and 12) were too large. It was rather difficult to be entirely sure that the perspective of the entire figure was completely reversed each time. In order to determine what sizes of the figure were most satisfactory, a number of the drawings of the cube varying in size from a one and one half inch face to an eight inch face were tried at different times by a number of observers. There was practically no difference in reversal time for the different sizes of the same figure by the same observer. For most observers Figs. 10 and 11 were most satisfactory for the cube. Of the various other reversible figures, Figs. 13 and 14 were chosen. These two, with either 10 or 11 (largely as the individual observer preferred) were used in all subsequent experiments.

Fig. 13 (the Scripture fluctuating blocks), while in one respect more satisfactory than the others, possesses another characteristic which makes it least satisfactory of all the figs. The advantage which it has over the others is that each perspective is always clear and distinct. There is no planospective view possible with it. The other characteristic which makes it less satisfactory than the others is the way in which it reverses. We have described the reversal of the perspective of an ambiguous figure as a process by which the spatial relations of the lines, angles, and shadows of a drawing, or other representation of an object are mentally exchanged or interchanged in such a manner as to induce a mental image or concept of the object represented either in a changed position, or with the relation of its parts changed, or with the observer's viewpoint with respect to the object changed. In reversing the figure of the pyramid or the cube the position of the entire object is changed. With one perspective it is as though we were looking at the object with its top and right face in view; with the other perspective the object is tipped backward and toward the right so that the bottom and left face are brought into view. With the figure of the stair the reversal consists in changing the observer's point of view or his position with respect to it. For one perspective he is looking at it from the front and somewhat above, while for the other perspective he is rather behind and under the stairway. For a number of our observers, in order to see the figure with this latter perspective, it was necessary to look at a real stair from the position suggested and compare the view thus secured with the figure. In all three of these figures, however, the objects represented remain unaltered in so far as the relations of their parts are concerned. The reversals consist simply in looking at different sides of the object.

With the Scripture blocks the process of making the reversals is of a different character. Parts of the figure that belong to one block or tier of blocks when seen with one perspective are made to belong to another when seen with the other perspective. For this reason it is difficult, while looking at the figure with one perspective, to recall just how it will appear with the other. Some of the observers experienced considerable difficulty in acquiring any definite technique or method in making the reversals. They came rather fortuitously. The process was somewhat like working a chain puzzle. The parts can be separated by certain definite manipulations, or often simply by shaking it promiscuously. Each observer, however, was urged to try to make the reversals in the same way each time and thus eliminate as far as possible the chance or fortuitous elements from the experiments. The results with this figure, while less uniform than with the others, are sufficiently so to indicate that, in so far as the reversals were made in a regular way, they were slower when the observer was fatigued than when he was rested.

The next group of experiments, twenty-six in all, were made with a view to determining the relative time necessary to recover from the fatigue induced by work periods of varying lengths. In these experiments eight observers worked one half hour; eleven worked one hour, six one and one half hours, and one worked two and one half hours. In each case the work consisted in doing the mental additions as in groups one and two of the preceding experiments. The reversal time for three different figures was taken just before beginning the work, as in the other experiments, again, immediately at its close and then at intervals of five to twenty minutes until the initial reversal time was approximately attained; the observer doing no work at all during these latter intervals. The three figures used in these experiments were the cube (either Fig. 10 or 11), the fluctuating blocks, and the stair.

Inasmuch as all the records of the different tests for each individual observer were made on the same sheet of smoked paper, without removing it from the drum, it was not possible to tell, at the time the experiments were made, just when the initial speed of reversing the different figures had been attained. Besides, this initial reversal speed, or reversal time, for some of the figures was attained sooner after the work period than for other figures. It was, for this reason,

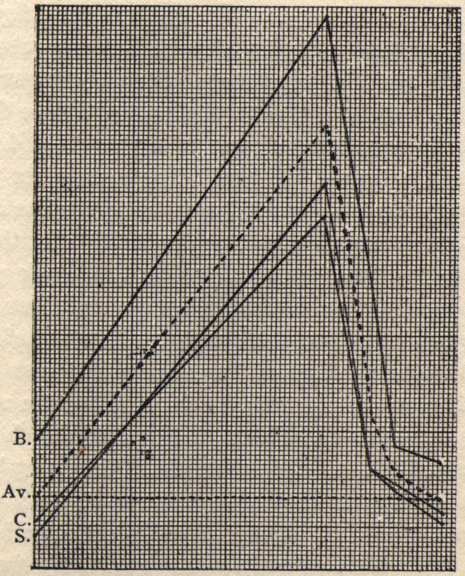


FIG. 15.

necessary to devise some uniform method for calculating the recovery time by using the average of the result obtained for all the figures. This method was as follows: On a sheet of cross section paper a curve was drawn showing the reversal time for each figure at each separate test and also the time in minutes between the tests in the different experiments.

Fig. 15 will illustrate the method employed. Each horizontal line in this figure represents one twentieth of a second and each perpendicular line one minute. The reversal time is thus represented, by the horizontal lines, in twentieths of a second, and the time between tests, by the perpendicular lines, in minutes.

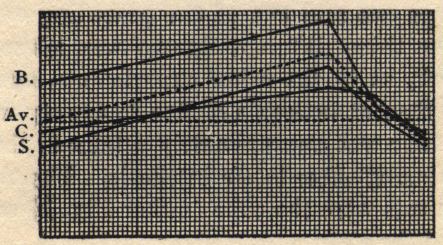


FIG. 16.

The letters B. C. S. and Av. at the left margin show the reversible figure used. B. = blocks (Fig. 13); C. = cube (Fig 10 or 11) (in this instance, Fig. 11); S. = stair (Fig. 14); Av. = average for all three figures. The reversal time for the different figures before any work was done,—for the blocks 1.9 sec., for the cube, 1.05 sec., and for the stair, .9 sec. The average for all figures being 1.3 sec. After one hour's mental addition the reversal time for the different figures was,—for the blocks 6.4 sec., for the cube, 4.25 sec., and for the stair 4.6 sec.; average, 5.2 sec. The next tests were made ten, fifteen and twenty-five minutes respectively after the one immediately following the one hour's addition. In Table IX. the reversal time for not more than three tests following the work period is given. In those experiments in which four tests were given, one is omitted from the table and the rest periods immediately before it and after it are added together and given as one single rest period. In the first of these, the one coming ten minutes after the work period, the reversal time was,—for the blocks, 3.5 sec. for both cube and stair, 1.6 sec.; average, 2.2 sec. In the second "recovery" test taken fifteen minutes after the work period, the reversal time had decreased to 1.85 sec., for the block, 1.45 sec., for the cube, and 1.4 sec., for the stair; average, 1.6. In the last test taken twenty-five minutes after the work had ceased, the reversal time was,—for the block, 1.7 sec., for the cube, 1.15 sec., and for the stair, 1.05 sec.; average, 1.3 sec., which in this case chanced to be the average reversal time for the initial test. Hence, by this experiment twenty-five minutes was found to be the time required for this subject to recover entirely from the fatigue of one hour's mental addition.

In very few of the experiments, as previously stated, was the reversal time in the last test exactly the same as in the first. For example, in the experiment represented in Fig. 16, the initial reversal time was 1.6 sec., for the blocks, 1.1 sec., for the cube, and .95 sec., for the stair; average 1.2 sec. In the last test, taken twenty minutes after the period of work, the reversal time for the blocks was 1.05 sec.; for the cube, 1 sec., and for the stair, .95 sec.; average, 1 sec. This reversal time was .2 sec. shorter than in the preliminary tests. In order then to determine the length of the rest period after one hour's mental addition which would just suffice to regain the initial average reversal time, a line was drawn parallel to the base of the figure through the point on the left hand margin which indicated this initial time. The point of intersection of this line with the curve of the average was taken to represent the point on the curve at which the initial reversal time was attained. In this case the point of intersection is on the fifteenth perpendicular line, counting to the right from the line which marked the first test after the work period. Fifteen minutes are accordingly taken as representing the time necessary for this observer to recover from the fatigue induced by one hour's mental addition.

In other experiments, the reversal time in the last test made was not as short as in the initial test. In such cases the curve of the average reversal time for all the different figures was produced until intersecting the horizontal line which marked the initial reversal time. In all cases the point of intersection of the *average* curve with the horizontal line which marked the initial reversal time was taken as indicating the recovery time for the particular work period of the experiment.

While this method of calculating recovery time is entirely arbitrary and may not, and probably does not, in every case represent the exact time necessary to recover from the fatigue of the work done in the experiment, it probably comes nearer to determining it than any other method making use of this form of experiment for measuring fatigue. The facility in making the reversals acquired in the practise afforded in the successive tests of the experiment would tend to aid one in regaining his initial reversal speed before he had entirely recovered from the fatigue brought on by the work of the experiment. This conclusion would seem to be borne out by the fact that in a number of the experiments the reversal time for the last test was shorter than in the preliminary test.

The results of this group of experiments will be given in Table IX. In Column I., Table IX., is given the observers' initials; in Column II. the reversible figure used; in Column III. the reversal time for the initial test; in Column IV. the reversal time for the first test after the work period; in Column V. the length of the first rest period; in Volumn VI. the reversal time for the second test following th work; in Column VII. the second rest period; in Column VIII. the reversal time for the third test after the work period; and in Column IX. the recovery time as calculated by the method previously described.

In those experiments in which the work period was but one half hour long the results seemed rather inconclusive. The period of work was too short to induce any very perceptible fatigue. In one experiment it appeared to serve as a sort of warming up process, the reversal time after the work being shorter than before.

This "recovery time" here given is exclusive of the time spent in making the tests. Each test occupied from two to three minutes, that is, about forty to sixty seconds for each reversible figure.

It is not possible that this "recovery time" represented, in every case, the exact account of time required for the observer to recover from the specific amount of work done in the experiment. For the majority of the observers this was their first experience with the reversible figures. The practise in making the reversals in successive tests would, of course, tend to increase their speed and make it possible to attain the speed with which they could reverse the figures before the work period in the experiment before they were entirely recovered from the fatigue of the work. The fact that practise did tend to increase the speed of reversals is clearly indicated by the fact that a number of observers attained a more rapid rate in the third or fourth test after the work than in that of the initial test.

Another factor which probably influenced the results of these experiments is that they were made during the regular university sessions with students who were carrying on their regular work. Many of the experiments were made late in the day, and in some cases by students who had had a rather strenuous day's work before coming to the experiment. In three of the experiments in which the work lasted one hour the observers expressed themselves on coming to the experiments as being about "done up" and fit subjects for fatigue experiments without further work. Two of these required the shortest time in which to regain their initial speed of reversing the figures after the work period of any in the entire "hour" group. In these cases the control over mental processes necessary to reverse the figures rapidly was somewhat disorganized when they came to the experiment and only a short rest after the hour's additional work was sufficient to regain the measure of control they had at its beginning.

TABLE IX

EXPERIMENTS WITH HALF-HOUR WORK PERIOD

Observer	Fig.	Prelim. Test (Sec.)	1st End Test (Sec.)	1st Rest (Min.)	2d End Test (Sec.)	2d Rest (Min.)	Last End Test (Sec.)	Recov. Time (Min.)
K. C.	10	1.22	1.43	5	1.45	5	1.29	
K. C.	13	1.70	2.35	5	1.38	5	1.59	
K. C.	14	1.55	3.54	5	1.72	5	1.38	
Av. for 3 figs.		1.48	2.44	5	1.52	5	1.41	8
K. C.	10	.96	1.09	5	1.24	4	.94	
K. C.	13	1.45	1.80	5	1.82	4	1.70	
K. C.	14	1.18	1.34	5	1.34	4	1.11	
Av. for 3 figs.		1.20	1.41	5	1.47	4	1.25	10 +
J. G.	10	1.63	1.80	12	——	—	1.48	
J. G.	13	3.36	3.36	12	——	—	2.13	
J. G.	14	1.64	2.08	12	——	—	1.40	
Av. for 3 figs.		2.21	2.41	12	——	—	1.67	6
J. G.	10	1.32	1.64	8	——	—	1.51	
J. G.	13	2.00	3.14	8	——	—	1.92	
J. G.	14	1.27	1.59	8	——	—	1.33	
Av. for 3 figs.		1.53	2.13	8	——	—	1.59	9
E. S.	10	4.33	2.56	6	4.30	6	1.68	
E. S.	13	3.33	3.07	6	4.19	6	1.79	
E. S.	14	2.21	2.80	6	3.07	6	1.78	
Av. for 3 figs.		3.29	2.81	6	3.85	6	1.75[3]	
E. S.	10	1.36	1.48	10	——	—	1.30	
E. S.	13	1.18	1.81	10	——	—	1.57	
E. S.	14	1.36	1.64	10	——	—	1.50	
Av. for 3 figs.		1.30	1.64	10	——	—	1.45	13
E. S.	10	1.23	1.71	12	——	—	1.28	
E. S.	13	1.42	1.50	12	——	—	1.20	
E. S.	14	1.31	1.49	12	——	—	1.23	
Av. for 3 figs.		1.32	1.57	12	——	—	1.40	17
K. M.	10	2.25	3.15	12	——	—	2.09	
K. M.	13	1.64	2.28	12	——	—	2.50	
K. M.	14	1.69	3.14	12	——	—	2.42	
Av. for 3 figs.		1.86	2.86	12	——	—	2.34	18

Average recovery time for seven subjects 11.5

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[3] In this experiment the reversal time was slower for the preliminary test than for that immediately following the work period; hence, no recovery time could be calculated.

Experiments with One Hour Work Period

Observer	Fig.	Prelim. Test (Sec.)	1st End Test (Sec.)	1st Rest (Min.)	2d End Test (Sec.)	2d Rest (Min.)	Last End Test (Sec.)	Recov. Time (Min.)
D. L.	11	1.05	4.25	10	1.60	15	1.15	
D. L.	13	1.90	6.40	10	3.50	15	1.70	
D. L.	14	.90	4.60	10	1.60	15	1.05	
Av. for 3 figs.		1.30	5.20	10	2.23	15	1.30	25
H. B.	10	1.10	1.55	10	1.50	10	1.00	
H. B.	13	1.60	2.20	10	1.30	10	1.05	
H. B.	14	.95	1.70	10	1.25	10	.95	
Av. for 3 figs.		1.21	1.80	10	1.35	10	1.00	15
A. R.	10	1.95	3.75	6	2.61	16	1.45	
A. R.	13	1.28	2.88	6	2.04	16	1.40	
A. R.	14	1.87	4.45	6	2.63	16	1.39	
Av. for 3 figs.		1.70	3.69	6	2.43	16	1.41	14
A. R.	10	1.00	1.28	18	——	—	.67	
A. R.	13	.72	1.25	18	——	—	.67	
A. R.	14	.69	1.38	18	——	—	.50	
Av. for 3 figs.		.80	1.30	18	——	—	.61	18
F. K.	11	1.81	2.80	18	——	—	1.79	
F. K.	13	3.33	8.00	18	——	—	3.60	
F. K.	14	1.40	2.03	18	——	—	1.44	
Av. for 3 figs.		2.18	4.28	18	——	—	2.28	19
F. D.	10	1.33	2.30	10	1.65	10	1.38	
F. D.	13	2.26	3.07	10	2.35	10	2.45	
F. D.	14	1.16	1.62	10	1.42	10	1.26	
Av. for 3 figs.		1.58	2.33	10	1.71	10	1.70	22
F. D.	10	1.25	2.00	10	1.85	15	1.32	
F. D.	13	2.09	2.83	10	2.17	15	2.09	
F. D.	14	1.26	1.89	10	1.79	15	1.50	
Av. for 3 figs.		1.53	2.24	10	1.94	15	1.64	23
A. O.	10	1.41	2.09	10	1.67	12	1.31	
A. O.	13	4.10	6.16	10	6.43	12	3.83	
A. O.	14	1.48	2.15	10	1.81	12	1.27	
Av. for 3 figs.		2.33	3.47	10	3.30	12	2.14	22
A. O.	10	.85	1.55	21	——	—	.83	
A. O.	13	1.96	3.20	21	——	—	1.73	
A. O.	14	1.00	1.47	21	——	—	1.02	
Av. for 3 figs.		1.27	2.07	21	——	—	1.19	18
I. P.	10	3.15	7.17	10	6.26	14	2.88	
I. P.	13	2.38	8.60	10	3.14	14	2.32	
I. P.	14	1.83	6.00	10	2.50	14	2.04	
Av. for 3 figs.		2.45	7.26	10	3.98	14	2.41	21
I. P.	10	4.80	7.00	10	5.89	15	4.90	
I. P.	13	4.70	6.25	10	8.83	15	4.46	
I. P.	14	3.61	6.13	10	5.10	15	4.08	
Av. for 3 figs.		4.37	6.61	10	6.46	15	4.48	22

Average recovery time for eleven subjects 20

Experiments with One and One-Half Hour Work Periods

Observer	Fig.	Prelim. Test (Sec.)	1st End. Test (Sec.)	1st Rest (Min.)	2d End. Test (Sec.)	2d Rest (Min.)	Last End. Test (Sec.)	Recov. Time (Min.)
E. P.	11	1.50	2.10	20	1.65	15	1.45	
E. P.	13	1.95	5.00	20	3.00	15	2.25	
E. P.	14	1.37	2.15	20	1.60	15	1.40	
Av. for 3 figs.		1.61	3.08	20	2.08	15	1.70	37
I. A.	10	1.50	2.00	12	1.83	13	1.72	
I. A.	13	1.61	2.20	12	1.82	13	1.66	
I. A.	14	1.53	2.40	12	1.90	13	1.64	
Av. for 3 figs.		1.55	2.20	12	1.85	13	1.67	28
F. C.	11	1.73	2.27	15	2.18	10	2.18	
F. C.	13	3.37	6.11	15	6.25	10	3.43	
F. C.	14	1.40	2.20	15	1.77	10	1.45	
Av. for 3 figs.		2.17	3.53	15	3.40	10	2.35	27
J. W.	11	2.06	4.22	20	2.17	10	2.37	
J. W.	13	2.73	6.16	20	3.50	10	2.62	
J. W.	14	2.17	5.29	20	3.09	10	3.00	
Av. for 3 figs.		2.32	5.22	20	2.92	10	2.67	34
R. B.	11	1.65	4.45	20	1.95	10	2.10	
R. B.	13	2.15	4.35	20	2.15	10	1.70	
R. B.	14	1.55	6.25	20	3.00	10	2.90	
Av. for 3 figs.		1.78	5.02	20	2.35	10	2.23	35
J. D.	11	2.35	5.50	15	3.93	10	2.00	
J. D.	13	3.15	7.57	15	4.14	10	3.35	
J. D.	14	1.91	2.20	15	1.68	10	1.24	
Av. for 3 figs.		2.47	4.76	15	3.25	10	2.20	22
Average recovery time for six subjects								30.5

Experiment with Two and One-Half Hour Work Period

Observer	Fig.	Prelim. Test (Sec.)	1st End. Test (Sec.)	1st Rest (Min.)	2d End. Test (Sec.)	2d Rest (Min.)	Last End. Test (Sec.)	Recov. Time (Min.)
I. A.	10	1.53	2.89	15	1.85	24	1.54	
I. A.	13	1.54	2.87	15	1.96	24	1.71	
I. A.	14	1.57	2.88	15	1.92	24	1.62	
Av. for 3 figs.		1.55	2.88	15	1.91	24	1.66	47

The relation of "work period" to "recovery time" as shown by the preceding experiments is very different from that found by Mosso in his experiments with the ergograph. His results showed that as he increased the work period in an arithmetical ratio, the period for complete recovery would have to be increased in geometrical ratio. That is, if any definite work period were doubled, it would require four times as long a rest period to recover. This difference in results is, it seems to me, susceptible of either or both of two possible explanations: (1) These two methods of experimentation measured two different phases of fatigue. Mosso's experiments measured the loss in capacity of the muscles to make contractions. My experiments measure the loss of control over the direction of nervous excitation. (2) Muscular exhaustion involves certain changes in the muscle tissues,—losses which must be replaced and the formation of poisons which must be removed. Loss of control involves only a kind of disorganization which, evidently, would be corrected much more rapidly than the losses in muscle fiber could be repaired and waste matter removed.

It is also wholly probable that a much shorter work period immediately following the rest periods which were found sufficient to recover from the fatigue of any particular work period would induce an equal degree of fatigue to that resulting from the first work period. This is, of course, wholly a conjecture and has not been demonstrated.

The next group of experiments was made upon observers who had a definite work program for an entire day[4] and adhered to this program without deviation. They began work at nine o'clock in the morning and worked without interruption until noon. There was then a rest period of two hours followed by another period of work from two to five. Tests in reversal time (for the three figures used in the previous experiment) were made at nine, ten, eleven, twelve, two, three, four, five, and seven o'clock; no work being done between five and seven. Four of the observers were instructors or assistants in the university. Their work consisted in marking quiz papers and special topics submitted by pupils in connection with their regular work. The other two observers were senior students in the department of education. One of these spent the day in typewriting a thesis, a form of work which he said was very taxing. He had not operated a typewriter long and the work had not become mechanical, but on the contrary required the constant concentration of attention. The other observer devoted the day to writing up, from notes previously prepared, a special paper.

The results of this group of experiments are given in Table X. In Column I. of the table is given the observers' initials as in the other tables; in Column II. the reversible figure used in the tests, and in the other nine columns successively the reversal time at nine, ten, eleven, twelve, two, three, four, five, and seven o'clock in order.

In this and in the two following groups of experiments the rate, not only of the voluntary or "forced" reversals was tested but also of the spontaneous or fortuitous as well. For the latter the observers were instructed simply to look steadily at the figure regardless of which perspective they should see. They were not in any sense to try to control the reversals, but were to allow them to come entirely spontaneously. The results of these tests in spontaneous reversal time are given in Table XI. In each test the observer would, for each figure, make the reversals voluntarily and as rapidly as possibl for about thirty-five to fifty seconds, and then at a signal from the experimenter would immediately take a passive attitude toward the figure and record the reversals as they came spontaneously, for approximately an equal length of time. It is these spontaneous reversals that are recorded in the following table.

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[4] One subject worked but half a day.

TABLE X

Obs'r	Fig.	9:00 (Sec.)	10:00 (Sec.)	11:00 (Sec.)	12:00 (Sec.)	2:00 (Sec.)	3:00 (Sec.)	4:00 (Sec.)	5:00 (Sec.)	7:00 (Sec.)
J. D.	10	1.28	1.10	1.10	1.43	.84	1.20	1.28	1.52	1.20
J. D.	13	2.60	3.00	2.84	3.12	2.82	2.45	2.61	2.69	2.61
J. D.	14	1.17	1.16	1.33	1.49	1.08	1.18	1.48	1.65	1.17
I. A.	10	1.57	1.57	2.33	2.80	1.55	1.77	2.37	2.62	1.57
I. A.	13	1.77	2.20	2.69	2.37	2.16	2.47	2.61	2.77	2.09
I. A.	14	2.00	2.05	2.21	2.78	1.65	2.25	2.47	2.69	1.60
F. C.	10	1.29	1.43	1.52	1.68	1.44	2.08	2.18	2.09	——
F. C.	13	2.26	2.15	1.80	1.41	1.59	2.37	1.96	2.18	——
F. C.	14	1.26	1.57	1.72	1.84	1.50	2.09	2.00	2.05	——
B. W.	10	.73	.71	.84	1.04	.74	.89	1.12	1.21	.89
B. W.	13	1.25	1.08	1.24	1.43	1.17	1.37	1.61	1.85	1.41
B. W.	14	1.06	1.04	1.09	1.38	.94	1.15	1.61	1.66	1.10
H. H.	10	.85	.78	.96	1.06	.79	.93	1.06	1.19	.79
H. H.	13	2.33	2.67	3.06	3.40	2.17	2.89	3.25	3.39	2.27
H. H.	14	.82	.74	.93	1.09	.83	.96	1.04	1.17	.79
D. S.	11	——	——	——	——	1.24	1.55	1.71	1.91	1.33
D. S.	13	——	——	——	——	1.67	2.20	2.44	2.61	1.77
D. S.	14	——	——	——	——	1.50	1.72	2.19	2.44	1.57
Av.	—	**1.48**	**1.55**	**1.71**	**1.89**	**1.43**	**1.75**	**1.89**	**2.09**	**1.48**

All the observers in these experiments were entirely familiar with the process of reversing the perspective of the figures as they had all acted as observers in one or more previous experiments; three of them having, only a few days before, gone through a practise series of tests taken at intervals throughout the whole day from about seven in the morning till ten at night.[5]

In all these experiments there was a slowing up in reversal time for the voluntary reversals (and in a part for the spontaneous) toward noon. After the two hour rest from twelve till two the rate of reversals was about equal to, and in some instances faster than, that for the first morning tests. The variation for the afternoon practically duplicated that for the morning.

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[5] This group of experiments was the last made in the study, but is reported at this point because the two remaining groups, yet to be discussed, were made, not for the purpose of measuring fatigue, but to test the effects of practise on the speed of reversals.

TABLE XI

Obs'r	Fig.	9:00 (Sec.)	10:00 (Sec.)	11:00 (Sec.)	12:00 (Sec.)	2:00 (Sec.)	3:00 (Sec.)	4:00 (Sec.)	5:00 (Sec.)	7:00 (Sec.)
J. D.	10	5.20	3.60	5.00	3.40	2.83	2.78	2.88	3.46	3.00
J. D.	13	5.78	7.43	5.20	3.78	3.64	4.55	4.46	4.30	3.23
J. D.	14	2.52	3.50	3.23	3.00	2.74	3.21	3.07	2.71	2.63
I. A.	11	4.00	3.14	3.75	4.17	2.87	3.58	5.25	4.00	2.69
I. A.	13	4.18	3.67	4.60	3.14	4.20	3.78	4.10	4.40	4.10
I. A.	14	4.40	3.75	2.94	3.21	2.69	3.00	3.75	3.75	3.75
F. C.	10	2.35	2.37	2.76	3.00	3.13	4.27	3.83	3.55	——
F. C.	13	2.81	3.13	3.13	2.24	3.07	3.14	3.92	3.92	——
F. C.	14	2.37	2.76	2.88	2.82	3.69	3.66	4.30	3.75	——
B. W.	10	1.39	1.59	1.12	1.50	1.26	1.53	1.48	1.85	1.73
B. W.	13	1.57	2.37	2.09	1.33	1.92	2.14	2.04	2.17	1.89
B. W.	14	1.96	1.63	1.64	1.83	1.74	2.04	2.00	2.43	2.00
H. H.	11	2.24	2.00	2.58	3.20	2.53	3.27	2.94	3.00	3.19
H. H.	13	3.62	3.06	3.36	3.69	3.83	3.92	5.11	3.92	3.31
H. H.	14	2.53	2.35	2.53	2.45	3.07	4.09	3.13	3.69	3.31
D. S.	10	——	——	——	——	3.42	2.30	2.42	2.24	3.20
D. S.	13	——	——	——	——	2.56	3.14	3.31	4.09	3.60
D. S.	14	——	——	——	——	3.54	2.39	2.71	2.71	2.44
Av.	—	**3.13**	**3.09**	**3.12**	**2.85**	**2.93**	**3.16**	**3.37**	**3.33**	**2.94**

Note.—The specific bearing of these spontaneous reversals upon this study will be given in the section dealing with the principles of the reversible perspective.

IV. The Effects of Practise in Reversible Perspective

The next two groups of experiments, though made before those just previously discussed are given last in the list of experiments because they were made for the purpose of testing the effects of practise on the speed with which the different figures could be reversed. In the first group (the results of which are given in Tables XII. and XIII.; XII. for voluntary and XIII. for spontaneous reversals), five observers took the tests at intervals of one to two hours from seven to nine in the morning until ten at night. With one exception these observers were entirely unfamiliar with the whole question of the reversible perspective. Tests in the rate of both the voluntary and spontaneous reversals were made. The experiments on the whole were not entirely satisfactory as practise experiments, since each observer had a different program for the day. All of them did more or less work during the day, but with one exception this work was very promiscuously distributed throughout the day. This observer, S. S., worked continuously from 8:45 in the morning until 12:00. Then again from 1:40 in the afternoon until 4:20, and from 7:30 until 9:30.

In Column I. of Tables XII. and XIII. are given the observers' initials; in Column II. the reversible figures used in the tests; and in the subsequent columns the reversal time for the various figures for the different tests; Column III. containing the records of all tests coming between 7:00 and 9:00 in the morning, and so on.

TABLE XII

Obs'r	Fig.	7:00 to 9:00 (Sec.)	9:00 to 10:00 (Sec.)	10:00 to 11:00 (Sec.)	11:00 to 12:00 (Sec.)	12:00 to 2:00 (Sec.)	2:00 to 3:00 (Sec.)	3:00 to 4:00 (Sec.)	4:00 to 5:00 (Sec.)	5:00 to 7:00 (Sec.)	7:00 to 8:00 (Sec.)	8:00 to 9:00 (Sec.)	9:00 to 10:00 (Sec.)
H. H.	10	2.75	3.15	3.38	2.28	1.38	1.04	1.18	1.25	1.38	1.05	1.07	1.34
H. H.	13	6.28	5.16	5.38	3.36	2.72	3.64	2.83	2.67	3.14	3.00	3.21	3.55
H. H.	14	2.00	2.22	2.50	1.48	1.25	1.19	1.33	1.42	1.37	1.04	1.07	1.33
F. C.	10	1.65	1.52	1.48	1.43	1.12	1.25	1.54	1.15	1.18	1.04	1.05	1.07
F. C.	13	5.55	1.87	1.62	1.64	1.35	1.64	1.45	1.42	1.55	2.04	1.24	1.19
F. C.	14	1.22	1.22	1.33	1.35	1.06	1.22	1.50	1.19	1.14	1.12	1.05	1.07
B. W.	10	1.42	1.22	1.02	1.07	1.04	1.00	1.13	.94	.93	.93	1.00	1.15
B. W.	13	2.30	1.95	1.52	1.29	1.18	1.26	1.38	1.14	1.22	1.14	1.70	1.79
B. W.	14	2.24	1.83	1.31	1.29	1.23	1.15	1.33	1.00	1.14	1.11	1.21	1.36
S. S.	10	1.55	1.82	2.73	3.00	1.82	——	2.26	2.62	1.25	1.00	1.33	1.69
S. S.	13	3.64	3.00	2.50	1.62	1.55	——	2.13	2.15	2.15	1.39	2.39	2.60
S. S.	14	1.86	1.02	1.20	1.44	1.08	——	1.37	1.37	1.15	.96	1.07	1.16
W. K.	10	——	1.92	1.95	1.61	1.56	1.43	——	1.16	1.09	1.13	1.13	1.21
W. K.	13	——	4.09	5.57	4.08	2.17	1.38	——	1.16	1.29	1.39	1.24	1.50
W. K.	14	——	1.88	2.00	1.92	1.61	1.32	——	1.20	1.22	1.38	1.33	1.47
Av.	—	**2.71**	**2.26**	**2.37**	**1.92**	**1.47**	**1.46**	**1.62**	**1.46**	**1.41**	**1.31**	**1.41**	**1.57**

TABLE XIII

Obs'r	Fig.	7:00 to 9:00 (Sec.)	9:00 to 10:00 (Sec.)	10:00 to 11:00 (Sec.)	11:00 to 12:00 (Sec.)	12:00 to 2:00 (Sec.)	2:00 to 3:00 (Scc.)	3:00 to 4:00 (Sec.)	4:00 to 50:0 (Sec.)	5:00 to 7:00 (Sec.)	7:00 to 8:00 (Sec.)	8:00 to 9:00 (Sec.)	9:00 to 10:00 (Sec.)
H. H.	10	3.00	2.37	1.73	1.86	2.05	1.66	1.74	2.17	2.15	2.13	1.75	1.96
H. H.	13	3.75	3.00	4.60	3.82	6.27	6.27	5.50	3.33	4.66	5.50	5.25	5.66
H. H.	14	2.73	1.82	2.22	2.26	2.47	1.77	2.12	2.50	2.26	1.75	2.62	1.67
F. C.	10	3.27	3.07	3.54	2.15	2.30	1.79	2.29	2.10	1.60	1.82	2.29	2.08
F. C.	13	2.06	2.86	2.35	2.93	2.09	2.35	2.20	1.62	2.44	2.13	2.66	2.33
F. C.	14	2.06	2.93	2.42	2.14	1.84	1.61	1.57	1.40	1.18	1.70	2.62	2.08
B. W.	10	2.46	1.90	1.67	1.55	1.31	1.50	1.62	1.41	1.35	1.44	1.67	1.82
B. W.	13	3.30	3.08	2.93	1.91	2.00	1.71	2.12	1.78	2.00	1.70	1.96	2.26
B. W.	14	3.00	2.50	2.39	2.39	2.04	1.52	1.63	1.68	2.00	1.57	2.05	2.09
S. S.	10	5.83	7.80	6.14	14.00	4.80	——	5.67	8.00	2.15	1.94	2.86	2.79
S. S.	13	4.37	4.00	3.07	4.30	5.22	——	3.89	5.50	3.00	5.67	4.44	3.13
S. S.	14	3.60	2.79	2.75	5.25	1.64	——	1.89	1.94	2.39	2.42	3.64	3.20
W. K.	10	——	2.73	3.27	3.46	2.37	2.65	——	1.50	1.64	1.55	1.56	1.71
W. K.	13	——	4.44	4.36	3.67	3.38	3.00	——	2.26	2.05	1.74	2.16	2.58
W. K.	14	——	2.80	3.00	2.87	2.25	2.42	——	1.75	1.63	1.55	1.58	1.77
Av.	—	**3.29**	**3.21**	**3.09**	**3.70**	**2.80**	**2.35**	**2.69**	**2.60**	**2.17**	**2.31**	**2.47**	**2.47**

Though not altogether satisfactory as practise tests these experiments show two rather distinctive results. First there is a decided increase in the rate of reversals (except for subject S. S.) particularly in the first three or four tests: Second the rate of reversals becomes more uniform for the different reversible figures. That is, the curves come closer together and run more nearly parallel toward the close of the day.

In the last group of experiments four observers were to take the tests, once a day, and as nearly as possible at the same hour each day, for ten consecutive days. The experiments were not entirely satisfactory since the observers failed to come regularly to the laboratory. Sometimes they would miss two or three days in succession or come at different times on successive days. In no case was the time between the first and last test of the series less than twenty-four days. Besides, their programs of work were not the same. On some days the tests followed a period of rest, and in others a period of work.

The results of these experiments are given in Tables XIV. and XV. In Column I. are given the observers' initials; in Column II. the reversible figures used; and in the other columns the reversal times for the different tests in succession. Table XIV. is for voluntary reversals, and Table XV. for spontaneous reversals.

TABLE XIV

Obs'r	Fig.	1st Test (Sec.)	2d Test (Sec.)	3d Test (Sec.)	4th Test (Sec.)	5th Test (Sec.)	6th Test (Sec.)	7th Test (Sec.)	8th Test (Sec.)	9th Test (Sec.)	10th Test (Sec.)
H. K.	10	2.00	1.52	1.89	1.28	1.22	1.43	1.19	.96	.96	.90
H. K.	13	3.00	1.77	2.32	1.37	2.09	1.71	1.79	1.88	1.61	1.46
H. K.	14	2.00	1.61	1.78	1.26	1.23	1.15	1.16	1.09	.88	.91
W. R.	10	2,31	2.00	2.30	2.56	2.04	2.35	2.10	3.15	2.14	2.47
W. R.	13	5.62	7.71	6.50	4.67	4.45	3.64	3.64	6.00	4.30	4.33
W. R.	14	2.81	2.36	2.48	1.85	1.63	1.76	1.64	2.87	1.59	1.94
S. K.	10	1.67	1.43	1.23	1.18	1.21	1.10	1.08	.98	.94	.92
S. K.	13	1.96	1.72	1.70	1.65	1.18	1.31	1.04	1.22	1.41	1.16
S. K.	14	1.46	1.41	1.37	1.37	1.15	1.28	1.18	1.13	1.02	1.05
H. S.	10	5.11	3.61	2.43	2.38	2.25	2.28	2.04	1.59	1.57	1.41
H. S.	13	2.47	3.33	4.57	3.17	3.21	3.80	2.68	2.00	2.40	1.81
H. S.	14	2.71	3.06	2.29	2.86	2.47	2.83	1.59	1.55	1.92	1.39
Av.	—	**2.76**	**2.63**	**2.57**	**2.13**	**2.01**	**2.05**	**1.76**	**2.00**	**1.73**	**1.65**

These experiments show two rather distinctive results: (1) Practise tends to increase, especially for the first few tests, the rate of reversals; (2) it makes this rate more uniform for the successive tests and also for the different figures for the same test.

TABLE XV

Obs'r	Fig.	1st Test (Sec.)	2d Test (Sec.)	3d Test (Sec.)	4th Test (Sec.)	5th Test (Sec.)	6th Test (Sec.)	7th Test (Sec.)	8th Test (Sec.)	9th Test (Sec.)	10th Test (Sec.)
H. K.	10	3.50	1.77	1.92	1.72	1.63	1.53	1.53	1.32	1.12	1.14
H. K.	13	3.60	1.69	2.67	2.16	2.20	2.08	2.00	1.84	1.64	1.71
H. K.	14	2.00	1.67	1.82	1.74	1.84	1.47	1.52	1.50	1.31	1.39
W. R.	10	6.16	4.50	3.78	4.55	5.11	4.46	4.50	5.33	4.17	6.55
W. R.	13	6.29	6.55	7.14	8.20	8.00	5.89	8.80	6.86	9.60	7.67
W. R.	14	4.22	5.22	4.70	4.20	4.70	3.42	5.00	4.55	4.60	5.00
S. K.	10	3.50	2.05	2.82	2.50	1.92	1.92	2.19	2.16	2.09	2.23
S. K.	13	2.67	2.80	2.40	2.20	4.30	3.21	2.39	3.33	3.13	2.53
S. K.	14	3.27	1.78	1.67	2.30	4.35	2.42	2.50	2.10	2.55	2.29
H. S.	10	4.78	4.18	5.00	4.40	5.55	5.00	3.14	2.61	2.25	1.96
H. S.	13	3.67	4.08	4.00	4.00	4.64	5.00	2.00	2.76	2.58	2.43
H. S.	14	3.91	2.94	3.36	4.62	3.12	3.21	2.81	2.16	2.50	1.96
Av.	—	**3.88**	**3.26**	**3.44**	**3.55**	**3.86**	**3.30**	**3.20**	**3.04**	**3.13**	**3.07**

PART III. PRINCIPLES OF THE REVERSIBLE PERSPECTIVES

ALTHOUGH our study has dealt primarily with the problem of fatigue, yet since our conclusions, in so far as they relate to mental fatigue, are based almost entirely upon its effects on another phenomenon which has been the subject of extensive psychological study and discussion, it has seemed proper that a brief statement of the different theories by which this phenomenon has been explained, as well as our own interpretation of it, should be appended. I have referred to the process by which the perspective of an ambiguous figure may be reversed as an act distinctly mental or psychical in its character.

Among the earlier writers on the subject of the reversible perspective, however, opinion was almost equally divided as to the character of the process by which the reversals are made. One school or group of writers represented by such well-known authorities as Neckar, Schroeder, Brewster and Wundt has held that these these reversals are dependent primarily, if not solely, upon purely physical or physiological factors as eye movements, fixation point, etc., which in some way modify the real visual sensation. The other group of writers, of which Mach, Hering, Wheatstone and Helmholtz are perhaps the best known representatives, hold that the process is distinctly psychological and that it depends upon such factors as the disposition of the mind to project its own mental states outward and the tendency to read form and meaning into all kinds of shapes, however formless they may be. With the more recent writers the tendency seems to be to abandon the view that the reversals depend upon eye movement. Wallin who has perhaps made as extensive and intensive a study of the subject as any other writer, spoke in his earlier writings (32) only in terms of ridicule and derision of the theory that the phenomenon could be interpreted in psychical terms, but in his most recent writings (33) he has turned, at least in part, to this theory as affording the most plausible interpretation of it.

Although the eye-movement theory seems to be pretty generally abandoned, there are still very radical differences of opinion as to just how the process should be explained. Becher (3) holds that the direction and distribution of attention, and the general mental "set," or attitude of will, are the chief determining factors. De

Boer (5) explained it in terms of association and the control of mental imagery. Flügel (8) holds that the perspective with which one sees the figure is determined by what he terms the ''direction of attention.'' Slaughter (26) and Bonser (4) maintain that ''the spontaneous reversals of an ambiguous figure are the results of a condition to be found in vaso-motor changes, and in the Traube-Hering waves.'' Woodworth (36) defines the perspectives themselves as simple ''concept qualities'' inherent in the figures themselves. He denies that the process of making the reversals is in any sense a kinetic or sensory one, as it would be if caused by eye-movements or the change in fixation point; nor will he admit that the different perspectives are different images which the figure suggests. The perspective or mental concept of the figure is simply the mental reaction which it as a sensory stimulus calls forth. That the figure may be seen with either of the two perspectives is due to the fact that it possesses two antagonistic qualities, either of which may determine the character of the mental response. McDougall (20), attacking the subject from another angle, has maintained that these reversals are due to the rapid fatiguability and the general instability which obtain in all higher levels of consciousness. ''Seeing'' the figure with one perspective causes a nervous discharge or excitation in certain centers of the brain. This discharge or excitation soon develops a resistance at the synaptic connections of the different neurons of the centers involved, to such a point that the excitation is shunted off into other centers whose activity means the ''seeing'' of the figure with the other perspective.

This latter view hardly seems tenable in view of the fact that in all our experiments with voluntary reversals and in the majority of those with spontaneous reversals, these reversals were made much more slowly when the observer was fatigued than when he was rested. If spontaneous reversals are to be explained on the basis of fatigue, it is not the fatigue developing in the areas or centers involved in seeing the figure with either perspective, but in suppressing or inhibiting the activity of those centers which would be involved in seeing it with the other. In other words the reversals come about not because the particular areas whose excitation gives one perspective have become exhausted, but on the contrary because of an inability to keep inactive the areas whose excitation gives the other perspective. To continue looking at the figure with one perspective, and at the same time knowing how to secure the other, is equivalent to holding the mind in a state of strained inactivity, when it is all primed or set as it were for activity. The fact that one knows there is another perspective possible acts as a challenge to search it out. The principle was rather tritely expressed by one of the observers in the foregoing experiments. In discussing spontaneous reversals she said: "I hardly believe they are spontaneous; I know there is another view of the figure and I also know how to get it, and I can't help looking for it." The principle is somewhat analogous to that involved if one were to give a toy to a child and tell him at the same time that he had something else for him. He would no sooner be in possession of the first than he would be, not only willing, but extremely anxious to relinquish it in order to secure the second.

My own view of the general character of the process agrees more nearly with that of Woodworth than with any of the others; although I am unwilling to concede that mental imagery plays no part. It is altogether probable that practised observers, and others whose mental processes or mental reactions are more direct will be able to dispense with images and deal with the figures in terms of their own features or characteristics, while others will find them indispensable in determining the kind of "mental reaction" which the figures shall call forth. The same principle is involved here as in learning to read a new language. The beginner can interpret words in the new language only as they suggest words of like meaning in his own. But with continued practise he is able eventually to omit this intermediate step and interpret the language directly.

One of our observers in discussing the reversal of the Scripture blocks said that her chief difficulty in "making them change" was "while looking at them one way I can't think of anything they will look like when I see them the other way." Another evidence that imagery was employed by a large number of the subjects, especially those unfamiliar with the reversals was the fact that it was very often necessary, in pointing out the different perspectives, to use real objects, such as a box for the cube or a real stair for the stair figure.

The fact that different mental reactions are possible to the same figure means that it is capable of stimulating to activity different centers of the brain. That is, the sensory excitation caused by the figure as a visual stimulus may reach either of two brain centers, and the perspective with which the figure is seen will depend on which centers are active. The observer's ability to determine arbitrarily just which perspective he will have depends upon his ability to control the direction of this nervous excitation. A large number of aids are of course possible in accomplishing this. Anything which causes a heightened activity in one center and lessens the activity of the other will bring about the appropriate mental response.

The number and variety of these different aids employed by our observers to make the reversals was sufficient to show that none of them could be called indispensible or determining factors. One, in describing the process by which he reversed the cube said that he conceived it as a sort of collapsible object of which either side could be shoved back through the other. Having gotten one perspective his method for securing the other was to conceive the rear surface as in some way being drawn through to the front. He said that when he wanted the rear surface to come to the front his method was to say "beneath his breath," "Come out!" Another said the figure reversed for her in the same way; but in order to accelerate the speed of reversals she conceived herself as having a string attached to one of the surfaces extending toward her and an elastic spring attached to the same side of the cube and fastened to something behind it. To make one reversal she "pulled the surface toward her" and to make the other she "allowed the spring to pull it back." Others saw the cube as though balanced so that they could tip one side or the other toward them. Another whose rate of reversing the blocks was much more rapid than most of the other subjects said she conceived the figure as a hot air register in the wall of a furnace-heated room. To reverse it she raised and lowered her hand under the table on which were the figure and the telegraph key, with which she recorded the reversals. This manipulation of the hand was to raise and lower a lever which opened and closed the register.

To reverse the figures of the stair most of the observers would raise the head slightly or at least elevate the eyebrows, as if to look upon an elevated surface, or rather to look more directly down upon a surface slightly in front in order to get the top view and then to turn the head to one side as if looking under something for the underneath view.

To prove that these movements had but very little if any influence in determining the rate of reversals, I requested the observers to make them more rapidly or irregularly. The result was a confusion. The reversals were slower when the movements were more rapid. Anything which served to divide attention interfered with the results. The general conclusion was that all these movements were motor accompaniments of the mental response or "mental reaction."

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